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# PSYCHOLOGICAL ANALYSIS AND THEORY OF HEARING.

BY  
HENRY J. WATT.

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## PSYCHOLOGICAL ANALYSIS AND THEORY OF HEARING.

By HENRY J. WATT.

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DURING the last few years rapid additions have been made to our knowledge of auditory sensations, their relations, and their causes. And the natural product of the new facts has come forth in various extensions and modifications of previous theories. One of the most striking features of these is the eagerness with which inspiration is borrowed

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from the visual system of qualities. But we must remember that vision is only one of the senses and in taking its complexity as a model we may fail to do justice to the claims of the other senses. I have attempted to satisfy these claims by a general consideration of all the senses and in doing so I was compelled to advocate a change in our treatment of sound<sup>1</sup>. It is commonly held that the attribute of quality in the sense of sound is represented by what we commonly call pitch. I proposed to see in this, however, not quality, but the auditory analogue of what we call 'local sign' or localisation in the cutaneous and visual senses. The attributes of sound were then to be: quality, of which, as in the sense of touch, there is only one form; intensity; pitch, generically named 'order'; voluminosity, the parallel of extensity in other senses; and the temporal attributes. On careful consideration of all the new facts and theories I venture to say that the extensions of auditory theory which build upon the common treatment of pitch as quality and which confine their search for inspiration by analogy to the sense of vision with its manifold qualities are entirely misleading and that the best guide to a theory of sound, as of every sensory experience, simple or complex, is a general consideration of the typical characteristics or attributes of all the senses in parallel, as far as they go. In this paper I propose to show that the analysis of auditory sensations I suggested is not only preferable to any other present theory, but is the only acceptable line of analysis.

### I. *The background of senses on which hearing stands.*

'Midst the details of our knowledge concerning the complex senses it must not be forgotten whence we obtain our pattern for their analysis. We certainly do not find it in vision, which itself needs more help than it can give. Our standard lies rather in the simplest of sensations. In them we read both the problem and the solution of all the others. These may be divided into two groups, making with the first group three in all. A brief statement of these three groups shows how the problems of hearing, which belongs to the third group, must be approached.

The first group consists of the cutaneous and visceral senses. There is no difficulty in identifying the attributes of all these. In spite of the existence of different systems of receptors for each sense, only one quality occurs in each sense. It may differ in intensity. But one

<sup>1</sup> See *This Journal*, 1911, iv. 143 ff.



quality in varying intensities would only constitute one sensation, not a sense. That is provided by the occurrence of differences of locality or local sign, which is an ordinal attribute. Along with it goes a continuative attribute, here known as extensity. By that is not meant a variable feature of area or surface, but a constant constitutive of area of any kind. Only by a variation of order along with constancy of extensity can a variable extent be thought to occur. In the cutaneous senses the minimal sensations we actually receive are very small extents and present practically no room for further differentiation of localities, at least as far as our phenomenal distinctions go. The temporal attributes may be neglected in this paper<sup>1</sup>.

Sensations of taste may be added to this group. Their attributes are identical with those of the cutaneous sensations in all respects but one, viz. that in taste we find four different qualities. But this difference presents no real difficulty. We have almost as much reason to see in the four qualities of taste four different senses as in the four different qualities of cutaneous sensation. What these reasons are need not be recounted here. At the same time we may readily admit that the four gustatory qualities are more akin to one another than are the other four. Thus arises the suggestion and the problem that the sensory qualities occur only in forms which differ from one another by a distinct step. The solution of this problem must be the preliminary to any problem as to the nature of the difference between qualities.

The second group of senses differs from the first clear and easy group in being obscure and somewhat difficult in the matter of attributes and in presenting a certain amount of complexity. The senses included in it are the muscular, articular and organic. The obvious form of variation of the first is intensity, while locality, extensity, and quality are all obscure. Quality is easily absolved by the recognition that, like those of the senses already mentioned, it does not really vary. Locality or order varies from muscle to muscle, but not for any one muscle; and extensity is contained in the variation of the mass or bulk of the sensations which come from muscles of different size. In the articular sense we find a similar group of features, differently distributed however. For, while quality is likewise single, it is intensity which is obscure. The reason for this is probably merely want of

<sup>1</sup> For a detailed account of the distinction and justification of these attributes, see my previous papers in *This Journal*: (1) "The elements of experience and their integration," 1911, iv. 135 ff.; (2) "Are the intensity differences of sensation quantitative?" 1913, vi. 176 ff.; (3) "The main principles of sensory integration," *ibid.* 240 ff.

variation<sup>1</sup>. The only obvious variant in articular sensation is 'position,' which we may take to be its ordinal attribute. Extensity behaves here as in muscular sensation. It is implicit in the varying bulk, mass, or volume of the sensations from the joints, small and large. In organic sensations we find that obscurity and want of variation is fairly general, but with a little willingness all the attributes can easily be identified.

Two psychological problems are raised by the sensations of this group. The first is that of the presence and cause of the obscurity of an attribute. In these cases it may be referred readily enough to the absence of variation in the physiological correlate. Thus the organs of the articular sense are stimulated at all times by forces of practically constant intensity, while the organs of the muscular sense, if there are enough of them in each muscle to form a small system there, are all stimulated at once, not one at one moment and then another.

The second problem is that of compound sensations. In all the senses of this group we have good reason to suppose that many receptors are stimulated at once, as there are many in each muscle, round each joint, and in each proprioceptive mechanism of the body. This fact confirms the psychological conclusion that must be drawn from the mass, bulk, or volume that is inseparable from the corresponding sensations. Apparently we never get in isolation a single 'spot' of articular, muscular, or organic sensation, but only a mass or area of it. Area is familiar to us in cutaneous sensation and there is every reason to suppose that area consists there of a large number of neighbouring minimal (as we get them) sensations, which fuse to a continuous whole in virtue of the extensity that each possesses. There is in area no accentuation or discrimination of orders, unless by means of concomitant differences in the variable attributes of intensity, as also sometimes of quality (vision)<sup>2</sup>.

This theory of compound sensations may be extended to apply to the minimal sensation from the 'spot' of cutaneous sense, which may well be supposed to be only minimal for us, because the units of the receptor organs or their neural attachments make a smaller particulation of sensation impossible, and not because the 'spot' sensation does not really consist of still smaller psychological particles fusing into a tiny area in which no orders can be discriminated<sup>3</sup>.

<sup>1</sup> Cf. *This Journal*, 1911, iv. 159.

<sup>2</sup> Cf. my paper on "The psychology of visual motion," *This Journal*, 1912, v. 32 ff.

<sup>3</sup> Cf. the psychological theory of intensity offered by F. Brentano, *Untersuchungen zur Sinnespsychologie*, Leipzig, 1907, 53 ff.

The third group of senses raises all the difficulties and problems already mentioned and some new ones. The senses included are vision, hearing and smell. We find in the matter of attributes special cases of obscurity, difficulty, and complexity. The obscurity of intensity in vision raises a general problem regarding the primacy of intensity as an attribute and regarding its place amongst experiences, if it be secondary or derived<sup>1</sup>. The manifold and continuous variation of colour quality, as surveyed in the colour figure, raises anew the problem of compound sensations. Both of these appear again in the study of auditory sensations, which adds as a third the problem of a non-spatial, systemic order and continuity. In this paper I shall confine my attention to the problems of hearing.

## II. *Views concerning the quality of sounds.*

The feature of auditory sensation which till very lately has been generally classed as quality is pitch. As it varies continuously from lowest to highest tones, the number of qualities equals the number of distinguishable pitches. The smallest number of primary qualities to which this series of continuous variations can be reduced is two, of which each occupies one end of the phenomenal series. Such a reduction which treats the series of tones as the analogue of the series of visual brightnesses, was suggested by Mach<sup>2</sup>. But it is rejected by Stumpf and is not generally admitted. Each distinguishable tone is rather considered to be a simple elementary sensation, requiring, as in Helmholtz's view, a specific sense-organ. The number of these is therefore very great. Ewald has pointed out that the assumption of this correlation between a vast series of qualities and a vast series of sense-organs puts an enormous strain upon our conception of the biological evolution of hearing<sup>3</sup>.

Brentano<sup>4</sup> thought to remedy the deficiency of Mach's theory, while retaining its postulate of tonal primaries, by extending the analogy with vision. He therefore recognised in hearing a series of 'saturated elements'—the tones that lie within an octave. The repetition of octaves

<sup>1</sup> Cf. F. Brentano, *op. cit.*, and the physiological theory of intensity proposed by C. S. Myers, *This Journal*, 1913, vi, 137 ff.

<sup>2</sup> *Beiträge zur Analyse der Empfindungen*, Jena, 1886, 122. In this edition Mach uses the analogy of the series leading from red to yellow. This is in principle the same as the series leading from black to white.

<sup>3</sup> J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, LXXVI, 155.

<sup>4</sup> *Op. cit.* 101 ff.



was to be explained by reference to variation of the admixture of the brightness components of sound. But this theory, like Mach's, could not hope to be accepted on its own merits. For, although it explains various matters well enough, as any such theory may, it does not add enough to our insight and to our knowledge to make itself compelling, and it is not founded upon a mass of observations, as is that lately propounded by Révész and von Liebermann<sup>1</sup>. Their theory is practically identical with that of Brentano, except that what he calls difference of brightness, they call difference of *Höhe*<sup>2</sup>. But the observations made by von Liebermann on his own auditory sensations, as modified by a chronic paracusis, allow these authors to go far beyond the range of Brentano's views and make their theory much more convincing. For they have established the independent variation and recognition of these two aspects of tone. Their observations seem to me to be thoroughly consistent with all the known phenomena of hearing and therefore *prima facie* correct. Only their classification and theory of these aspects of tone appear to me to be impeachable and misleading. Their distinction explains much, of course; but as its basis in fact seems to be correct, proper classification and proper deductions therefrom should enable their theory to explain still more.

It is to be noted that Max Meyer<sup>3</sup>, whom Révész also quotes<sup>4</sup>, distinguishes the same two aspects of tones, but he calls them by reverse names. Révész's quality is his pitch and Révész's height is his quality.

In the hands of Köhler<sup>5</sup> and more especially of Jaensch<sup>6</sup>, Stumpf's generally accepted views have taken a different line of development. The chief influence here has been the observation of the resemblance between pure tones and vowels and also in the case of Jaensch consideration of the nature of the relations between the stimuli of tones, vowels and noises. The pure vowel sounds are for Köhler the sole qualities of hearing. For Jaensch they are only the qualities of the sense of noise. He recurs to a modified form of the analogy between vision and hearing in identifying vowels and brightnesses as less differentiated sub-senses. The stimulus for the former is a rate of

<sup>1</sup> Cf. the works quoted below under their names.

<sup>2</sup> This is the ordinary German word for our 'pitch'; for the sake of clearness, however, I shall translate it by its wider meaning—'height.'

<sup>3</sup> "On the attributes of sensations," *Psychol. Rev.* ix. 83, esp. 95 ff.

<sup>4</sup> *Zur Grundlegung der Tonpsychologie*, Leipzig, 1913, 42.

<sup>5</sup> *Ztsch. f. Psychol.* 1910, LIV. 241 ff. and 1911, LVIII. 59 ff.

<sup>6</sup> *Ztsch. f. Sinnesphysiol.* 1913, XLVII. 219 ff.



vibration which varies irregularly but not too extensively about a certain average, while the stimulus for the latter, although not necessarily irregular, is at least in the normal, light-adapted eye usually provided by certain pairs of lights or any mixture of these pairs. The more differentiated and presumably later developments—tone and positive colour—are evoked by steady rates of vibration and are likewise to be identified. The relation between tones and vowels, like those between colours and brightnesses, varies, sometimes being closer than at other times. Thus, presumably, we should have to admit a whole spectrum of qualities in the tones, and a series of qualities in the vowels—as it were octaves of brightness. Why there are different qualities in this series is not explained. And as Jaensch distinguishes vowels from noises, I can see no reason why we should not admit three sub-senses in hearing instead of merely two<sup>1</sup>.

The analysis into sub-senses is hardly attempted at all by Révész<sup>2</sup>. The only indication of it we get is the suggestion that the height of tones would need two psychophysical processes, while quality, as the evidence of binaural mixture of small pitch differences seems to show<sup>3</sup>, would also probably require two. If we add to these the differences of vocality which Révész recognises<sup>4</sup>, still more processes will be required and we shall quickly exhaust the resources suggested by even the most complex of visual theories.

The enormous influence of the analogy with vision upon these theories is obvious. I shall now proceed to summarize the facts and observations included in them and to interpret them according to my own theory of the attributes of sound stated above. It will then be evident which of all theories is the more systematic, that is free of difficulties and full of promise.

<sup>1</sup> Cf. *op. cit.* 240, 255, “Weichen die Schwingungszahlen der einzelnen Elemente eines Kurvenzuges allzu stark von einander ab, so wird aus dem Vokal ein Geräusch....Das undifferenzierte Geräusch ist somit die eine Klippe, welche bei der Herstellung eines Vokales vermieden werden muss; die andere Klippe ist der Ton.” If there are only two cliffs, there must be a valley between them, namely vowels. Or are there really three cliffs?

<sup>2</sup> *Zur Grundlegung der Tonpsychologie*, 41 ff.

<sup>3</sup> *Nachr. d. Gesell. Wiss. Göttingen, Math.-phys. Kl.* 1912, 676 ff.

<sup>4</sup> *Zur Grundlegung der Tonpsychologie*, 89, “Es gehört eben jeder Schwingungszahl eine Qualität, eine Höhe, und eine Vokale zu.”

III. *The relations of tones, vowels, and noises.*

There are pure tones and there are compound tones. The latter consist of many simultaneous tones which harmonize more or less with one another and with their fundamental component. If a tone does not last longer than the time of two vibrations, it is heard as a noise. Many tones of neighbouring pitch or generally inharmonic in relationship, sounded together, form a noise. The pitch of single noises is not very evident. But many noises contain distinguishable tones, and if noises are sounded one after another, their pitch becomes easily noticeable. Thus we obtain the propositions: (what is presumably) tone is sometimes heard as noise; some noises consist of (what are presumably) tones; some noises contain tones. The net result of these is the proposition: many (phenomenal) noises are, or consist of, (real) tones. In reliance upon this, it has been usually inferred that all noises consist of tones or are tones of very indefinite pitch or are not yet tones, so that there is no special sense of noise<sup>1</sup>. This conclusion seemed to be confirmed by the rather vague and conflicting statements made about the tonal nature of vowel sounds. The synthesis of sounds by experimental means did not admit of a direct examination of the proposition: are there any unanalyzable noises of indefinite pitch?

This experimental question has been answered by Jaensch by the use of a selenium cell placed in the circuit of a telephone and illuminated by an arc lamp whose light was varied by the revolution of an obstructing disc. The edge of this disc was cut out so that the variations in the length of its radius corresponded with the variations in height of any desired vibratory curve, pendular, periodic, or irregular. The results of these experiments are most acceptable. A constant rate of vibration produces a tone. The same average rate of vibration produces a vowel-like tone, if the mean variation from the average is still small. As the mean variation increases, the sound passes gradually into a vowel, then it takes on a noisy character, and when the mean variation is great enough, it may finally appear as pure noise. The average rates of vibration of the vowels *m*, *u*, *o*, *a*, *e*, *i*, and *s* are approximately octaves of one another<sup>2</sup>. These vowels do resemble certain tones, as Köhler maintains<sup>3</sup>. But they are not to be identified with them, as he proposes. The resemblance is close only between the lowest and highest

<sup>1</sup> Cf. C. S. Myers, *Textbook of Experimental Psychology*, 2nd ed., Cambridge, 1911 25 f.

<sup>2</sup> Jaensch, *op. cit.* 234 ff.

<sup>3</sup> *Op. cit.* esp. LVIII. 91 ff.

vowels and the corresponding tones; in the middle of the scale, from below the vowel *o* to above the vowel *e*, tone and vowel are easily distinguishable. Jaensch, therefore, ascribes vowels to a separate and older sense of noise, of which he supposes them to be primary qualities. Average rates of vibration that are greater than that of any vowel and less than that of the next higher one, form a series of mixed vowel sounds, which show a decreasing resemblance to the lower vowel and an increasing resemblance to the next higher vowel, as the average pitch rises. The changes from pure vowel to pure vowel thus obtained are parallel with the changes encountered as we pass from red to yellow, etc.<sup>1</sup>

Two statements of great psychological importance are involved in these views: (1) that hearing contains two psychologically independent sub-senses—tone and noise; (2) that *u*, *o*, *a*, and the rest are pure vowels, forming a series of qualities in the sense of G. E. Müller<sup>2</sup>.

The following objections have to be urged against the distinction of two sub-senses. It is supported by nothing more than analogy, and, at its best, that analogy is the analogy of stimuli, not of experiences. The stimuli of vowels and noises are irregular, those of tones are regular; the stimuli of colours are regular, those of neutral greys irregular. But it is to be noted that, while the former vary round an average, the latter go in pairs—those of the complementary colours. It is known that the sub-senses of vision exist independently; but there is no evidence that the sense of noise can exist without the sense of tone. And if there were such evidence, it would not be clear of ambiguity; for noises are not only, *ex hypothesi*, excited by tones, as brightnesses are excited by colours, but noises, when given, *ex hypothesi*, alone, resemble tones. It is true, as Jaensch propounds<sup>3</sup>, that each positive colour has an affinity to a neutral brightness, but it is not true, as his diagram suggests, that each neutral brightness has a resemblance to a colour, *quâ* colour; whereas each tone resembles (or according to Köhler is) a vowel, and on Jaensch's analogy must resemble it, because it excites it, and also each vowel resembles (or according to Köhler is) a tone or has the pitch of a tone, as Jaensch<sup>4</sup> has shown experimentally, and as all those who have attempted to find the component tones of vowels have observed. Moreover increase of intensity of light modifies a colour in

<sup>1</sup> Köhler, *op. cit.* LVIII. 99; Jaensch, *op. cit.* 258 f.

<sup>2</sup> *I.e.* "Eine Reihe von Empfindungen, in welcher sich die Qualität geradläufig [d. h. in konstanter Richtung vor sich gehend] und stetig ändert," *Ztsch. f. Psychol.* 1896, x. 33 ff.

<sup>3</sup> *Op. cit.* 264 ff.

<sup>4</sup> *Op. cit.* 288.



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the direction of greater neutral brightness, but increase of the intensity of sound does not bring a pure tone nearer to either vowel or noise. The sound-figure given by Jaensch<sup>1</sup> should be made tri-dimensional to suit the double parallel between the similarity of low and high tones to vowels and the varying similarity of different hues of different brightness to neutral greys, just as the tri-dimensional colour figure does. But this cannot be done for tone. The analogy of vision and hearing, tempting though it be, is both incomplete and misleading.

In any case, what sort of statements do the facts precisely warrant? The stimuli used show that, as we pass from tone through vowel to noise, the average rate of vibration remains constant, but its mean variation increases; in other words, the pitch wobbles; not markedly and noticeably, but none the less truly. We become less able to indicate the pitch by singing, or by naming it. But we can approximately find it (with a little circumspection and comparison) the more easily, the more it is isolated from accompanying (not from successive) tones, vowels or noises<sup>2</sup>. I see no reason to depart from the substance of the formulations regarding the relations of tones and noises referred to above. A noise may then be said to be a simple sound whose pitch is not yet audible or a complex sound of many pitches which make each other indistinguishable to the unaided attention. Or, to put it more briefly: tones, vowels, and noises vary from one another in respect of (decreasing) definiteness of their predominant pitch. Later on we shall see more how this statement can be founded and confirmed by both physiological and psychological theory.

The following objections have to be urged against the view that the series of vowels *m*, *u*, *o*, *a*, *e*, *i*, *s* and *ch* form a series of qualities in the sense of G. E. Müller.

It has long been an accepted view that the tones within the octave are not a mixture of the two end-tones of the octave. If the octave relationship and such others are ignored or suppressed, as for example by running in a single chromatic series through several octaves, all the tones passed seem to form part of a single series. If we suppressed the connexion between the octaves, as it holds in the slightly indefinite pitches of vowels, by somewhat similar means, should we not seem to pass through a single series of vowel sounds, in which there are no

<sup>1</sup> *Op. cit.* 265.

<sup>2</sup> Cf. Jaensch, *op. cit.* 288. A vowel produced apart from a fundamental tone has a pitch. "Die Tonhöhe erscheint hierbei im allgemeinen um so weniger deutlich, je grösser die mittlere Variation der Schwingungszahlen ist."



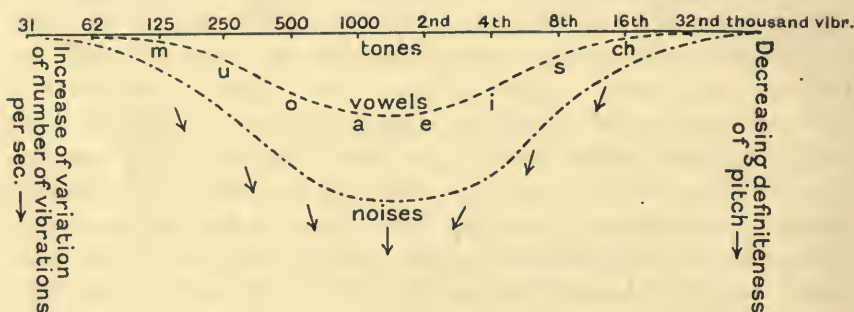
turning points? Amongst pure noises, the existence of which, as we have seen, Jaensch, like everyone else, must admit, no octave connexion of any kind is to be found. How then does it come to be present in the parallel series formed in the course of his experiments—pure tones, tone-vowels, and vowels, if not simply in virtue of the character which the stimuli suggest—the more or less precise average value of the vibratory frequency, the more or less definite pitch of the tones and vowels? The purity of the members of the vowel series would therefore by presumption rest upon the general octave relationship of sounds and not upon changes of quality in Müller's sense.

If the series of vowels consists of greatest differences whose stimuli differ by approximately an octave, we must not forget that the series of tones also contains an octave relationship of the greatest resemblance. It is not easy to reconcile this opposition, unless by the conversion of the 'greatest difference' given in the vowel series into a 'greatest resemblance amidst continuous difference,' just as is generally done for the pure tonal series. In Révész's view there is only a short series of different 'qualities' of sound, that covering the octave. The qualities within the octave differ the more, the greater the interval. Thus the greatest difference—the octave—is converted into the least difference, identity or repetition of quality. Only thus can we harmonize the older sense of noise with the younger sense of tone, as Jaensch calls them. Or surely Jaensch does not think that, if the series of vowels is the parallel to the series of neutral brightnesses, the series of pitches can be the parallel to the series of positive colours in any psychophysical sense? For if it were, we should have to see in the series of pitches a uni-dimensional series of qualities; or else we should have to split the bi-dimensional or tri-dimensional manifold into several uni-dimensional series, as is done in vision. In either case it would be difficult to find pure primary qualities. Even for Révész the method he suggests of deriving all the inter-octave qualities from two qualities is excluded; for his end qualities are greatest similars as well as greatest differences. If he wishes for greatest differences, he must seek them in tones that are just short of an octave apart. But to admit these as greatest differences would compel him to suppose that the greatest differences of all are next neighbours in the tonal series. These minutest of differences do not, of course, act as greatest differences; a slight mistuning of the octave is hardly noticeable. And then again no tones in any part of the octave give themselves out to be purer than any others.

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If the vowels of the series are to be taken as greatest differences, the fact that their pitch, in spite of its indefiniteness, is fixed round about an absolute point, becomes important. For the octave relation seems to hold only between the pure vowels, not between impure vowels which differ by approximately an octave. If this be so, it becomes difficult to see why the octave relation should be perfectly relative amongst tones, as it is. To postulate different senses, as Jaensch does, is too easy a way out of the difficulty. For both vowels and noises have a pitch, indefinite though it be.

We must therefore conclude that the series of vowels is a single linear series. If so, it is hardly necessary to bring the relations between tones, vowels, and noises into schematic form. But it might be done thus:



The only variant is pitch; it is higher or lower, and it is more or less definite. We have not far to seek for an analogous case of variation in definiteness of order, in the specific form of localisation; it is found in the differences between the epicritic, the protopathic, and the deep systems of cutaneous sensations. Besides a very acceptable physiological basis for increasing indefiniteness can be suggested in hearing.

The only question which remains is to explain why the vowels form a series of absolute octaves. When there is no known reason, any theory formed solely to explain this fact, as Jaensch's and Köhler's are, is as good as any other. Whether we explain octaves by the use of *ad hoc* qualities or not, we still have to explain why in pure tones the octaves are thoroughly relative, whereas for vowels they start from an absolute basis. If this absolute basis be denied, the whole distinction of pure vowels collapses. It is therefore quite as good a suggestion to suppose that the mouth for some reason or other chooses to form such a cavity for some one vowel that it gives a certain average tone, and that

the other vowel sounds are chosen, owing to the otherwise and already existing octave relationship, in relation to this primary vowel.

Our conclusion, then, thus far would be that there are in sounds differences of pitch of greater or less definiteness; that the only qualities involved in these differences are those of pitch, if these can be classed as qualities, or, if they are classed otherwise, as I believe they must be, that there are no differences of quality in sound at all; that there is only one auditory quality, namely sound.

#### IV. *The distinction of two aspects of tone within pitch.*

Various distinctions of this kind have been made. The most familiar recognises the variation of volume or voluminousness which accompanies change of pitch<sup>1</sup>. I think this distinction is also the most correct, and I have therefore adopted it<sup>2</sup>. Low tones are bulky and massive, high tones are thin, sharp and wiry. Sound offers in this respect a parallel to the bulk and volume of the second class of sensations, the obscure group. And we have good reason to extend to them the theory of compound sensations which applies to the articular, muscular, and organic sensations<sup>3</sup>.

Max Meyer and Révész also distinguish two aspects within pitch. The former calls them pitch (corresponding to the place in the musical scale) and quality (Révész's 'height'). Révész calls the differences of pitch within the octave 'quality' and an accompanying distinguishable difference he calls 'height.' I accept the distinction intended without the least hesitation, but it seems to me more correct<sup>4</sup> to class the differences intended as differences of 'pitch' and of 'volume.' Pitch, as I mean it, is Révész's quality, such differences as are given within the octave and are named by the letters *c, d, e, f*, etc. But I would not confine the range of these differences to those of a single octave, supposing, as Révész does, that the most similar qualities of different octaves are identical; I would recognise a regular variation of pitch from lowest to highest tones. Volume, as I intend it, is the difference intended by Révész's *Höhe* (height), a difference which changes with pitch throughout the whole range of tones, and which can be separately distinguished from pitch or order, and which can be compared with other degrees of volume. Révész and v. Liebermann have added very much to our knowledge of these properties of tones by

<sup>1</sup> Cf. Stumpf, *op. cit.* i. 207 ff., ii. 56 ff., 537 ff.

<sup>2</sup> This *Journal*, 1911, iv. 143 ff.

<sup>3</sup> Cf. This *Journal*, 1913, vi. 242 f.

<sup>4</sup> For reasons see This *Journal*, 1911, iv. 143 ff.



showing in such detail that pitch and volume (or, as they say, quality and height), are separately variable, to some extent in normal cases, and so completely in such an unusual case as v. Liebermann's. But I do not think that their theory of the limitation of qualitative differences to those that lie within the compass of an octave, contributes towards the explanation of the octave recurrences of tones. It introduces more difficulties than it removes. It is possible and it is better to place the octave recurrences as a special feature of tone to be explained by reference to the nature of the smallest complexes in which tonal sensations are given to us. To establish this view it will be necessary to make a somewhat detailed study of the facts reported by v. Liebermann and Révész<sup>1</sup>.

(a) *The hearing of single tones.* These were heard from a certain point of the scale onwards as a more or less constant pseudotone. The nature of the disturbance varied from time to time, often during one day, and sometimes even during a single observation, then giving rise to a gliding tone. But the main features of the disturbance were constant enough. As I take it, the disturbance consisted in some abnormal process affecting the basilar membrane or the contiguous parts involved in the reception of sounds; in its milder forms this made certain points or short extents within a larger extent of the receptors more sensitive to stimulation than the other parts of this extent; at the same time the effect produced upon the receptor itself was less intense than usual. In its severer forms a part of this region might be totally unreceptive. An analysis of the table given by von Liebermann and Révész<sup>2</sup> shows the following state. In general the region from about  $e^2$  to  $d\sharp^4$  or  $f^4$  is affected. On 20th and 27th April, 1907, there is in that region only one sensitive area,  $g\sharp^3$  (as such and *quâ*  $g^2$ ,  $g^4$ ), with sometimes a semitone higher and lower owing to assimilation or the halving of differences. On 17th October there are two almost equally sensitive points,  $f\sharp^3$  and  $c^4$  (also *quâ*  $f^2$  and  $f\sharp^3$ , or  $c^3$  and  $c^4$ ); of these  $f$  is affected by tones above  $f$  and below  $c$  in scale, while other tones

<sup>1</sup> The papers by these authors, Paul von Liebermann and Géza Révész, are: (1) "Ueber Orthosymphonie," *Ztsch. f. Psychol.* 1908, XLVIII. 259 ff.; (2) "Experimentelle Beiträge zur Orthosymphonie und zum Falschhören," *ibid.* 1912, LXIII. 236 ff.; (3) "Ueber eine besondere Form des Falschhörens in tiefen Lagen," *ibid.* 325 ff.; (4) "Ueber binaurale Tonmischung," *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 676 ff. These will be cited in the following pages as L. and R. 1, 2, 3, 4. Under the name of Géza Révész alone there have appeared: (1) "Nachweis, dass in der sog. Tonhöhe zwei voneinander unabhängige Eigenschaften zu unterscheiden sind," *Nachr. d. Gesell. Wiss. Göttingen*, Math.-phys. Kl. 1912, 247 ff.; (2) *Zur Grundlegung der Tonpsychologie*, Leipzig, 1913.

<sup>2</sup> L. and R. 1, 263.



affect  $c$ ; the region from  $c\sharp^4$  to  $e^4$  was dead. On October 29th the more sensitive point was  $g^3$  (also *quá*  $g^2$ ,  $g^4$ ), with some assimilation to  $a^3$ ;  $e^4$  was also sensitive. The same holds true of November 5th, only the dead region is now slightly alive. In these last thrice there is assimilation to tones lower than the most sensitive one only if the assimilating tone is on the lower edge of the affected area. The paracoustical region is not, as von Liebermann and Révész suggest<sup>1</sup>, broken by normal points, but only by points where the objective tone and the pseudotone coincide. The hypothesis of greater sensitivity fully explains the normality.

At a later date (1911) the state of the ear was of the same character, the most sensitive point was about  $g\sharp$ , but the area around it down to  $e$  and up to  $b$  was also sensitive. The extent of this sensitive area and the more sensitive points in it fluctuated from time to time. There was also for a time a sensitive  $c\sharp$  and  $d$ . Generally also tones lower than the lower end affect the low end of this area and higher tones the higher end<sup>2</sup>. This is the usual state of things and seems to occur when there is only one sensitive area with a most sensitive point.

It is important to notice that long-lasting tones, such as those of the violin and the flageolet, and also intense tones produce correct tones, while the brief tones of the piano produce pseudo-tones<sup>3</sup>. This bears out the idea of sensitive points; a full and proper stimulation is still able to affect the normal point.

(b) *Orthosymphony*. Here we have a suggestion for a theory of the phenomena called by von Liebermann and Révész 'orthosymphony,' i.e. that when two tones were sounded at once<sup>4</sup>, the interval was always correctly judged. To get the correct interval, however, it was necessary to take the chord as a whole without analysis. If the observer listened to get the single tones, he heard them as pseudotones. von Liebermann and Révész therefore supposed at first that both pseudotones were then really given. But later they believed and argued that this was not so; for "in dem Augenblick, wo das Heraushören gelang, wurde die dem richtigen Intervall eigentümliche Konsonanz nicht im geringsten verändert; die Verschmelzungsstufe blieb dieselbe, hinsichtlich des Intervallurtheiles trat aber Verwirrung ein, da die Vp. naturgemäss nicht im stande war, ein Urtheil zu geben über ein Intervall, das bei der Zerlegung

<sup>1</sup> L. and R. 1, 265.

<sup>2</sup> L. and R. 2, 308 (table).

<sup>3</sup> L. and R. 1, 264, 274.

<sup>4</sup> Or successively (arpeggios), i.e. physiologically and psychologically overlapping and therefore partially simultaneous, see L. and R. 2, 298. Also in melodies with accompaniments, *op. cit.* 299.

andere Komponenten lieferte, als nach dem Gesamteindruck zu erwarten war<sup>1</sup>." But these and all the other statements of the paper only show that the degree of fusion, not the pitches of the fusing tones, remained constant<sup>2</sup>. In fact everything points to the idea that in the *Gesamteindruck* other pitches were there than in the analysed impression or in the single tone. If one maintains that in the *Gesamteindruck* the false tones, though not heard, were really there, one might as well assert that when an intense tone or a continuous tone of the violin or flageolet evokes a correct tone, the pseudotone is really there, though it seems to be absent<sup>3</sup>. There can be no doubt that the pseudotone appeared in chords only when it was supported by the attention<sup>4</sup>, but that synthetically observed chords and intense and continuous tones, unsupported by the attention, were heard true. The abnormal sensitivity of the ear suppressed the normal sensitivity only when the latter was unsupported or briefly stimulated<sup>5</sup>.

(c) *Theory of the preceding*. It is not true that according to Ewald's theory or to any other we should have to say: the total impression is independent of the form of the sound picture<sup>6</sup> or of the actual effect of the stimulus on the receiving bodies; but we should have to qualify these theories by an addition to the effect that the *pitch* of a tone does not conform solely to the conditions laid down by the stimulus, either for single tones or for chords; doubtless it bears, as *e.g.* in Helmholtz's theory<sup>7</sup>, a specially close relation to the stimulative conditions; but this relation need not always be unmodifiable and final; the general expression, which includes it, will rather refer to the point of the basilar membrane that is stimulated most effectively, whether because of its own greater sensitivity, or because of the greater intensity of the stimulus, or because of the mutual support of several stimuli

<sup>1</sup> L. and R. 1, 270.

<sup>2</sup> Cf. L. and R. 2, 298.

<sup>3</sup> From a certain aspect of the theory which I shall later develop, both of these statements are true of the real components of tone, but for the present and from the phenomenal aspect both are surely false.

<sup>4</sup> Cf. L. and R. 1, 270. Pseudotone given first and attended to kept its pitch even when a second tone was sounded.

<sup>5</sup> Certain familiar facts of touch form a curious parallel to orthosymphony. In certain disturbances of the peripheral nerves of touch a single touch is very badly localised, whereas simultaneous points are discriminated from one another better than usual.

<sup>6</sup> L. and R. 1, 271.

<sup>7</sup> Helmholtz, *Sensations of Tone*, transl. A. J. Ellis, 3rd ed., London, 1895, 144, fig. 52. Cf. also the extensions suggested by A. A. Gray, *J. of Laryngology, Rhinol. and Otology*, 1905, xx. No. 6.

acting against the effects of greater sensitivity<sup>1</sup>, or because of the heightened sensitivity that attention gives to an already abnormally sensitive point, acting against the effect of mutually supporting stimuli. These various forces, which sometimes concur and sometimes oppose one another, suffice to explain all occasional exceptions to ortho-symphony, *e.g.* that melodies given with accompaniments are not affected by the attention<sup>2</sup>, and others.

Such a modification retains for the theory of hearing what is for very many reasons absolutely indispensable, namely a basis for the modification of analysis by means of the attention<sup>3</sup>. The pitch of a tone thus depends on the point of the basilar membrane that is for any or all reasons most effectively stimulated. The fusion of tones and the varying volumes of them upon which fusion rests, must therefore depend on the extents of the basilar membrane that are stimulated<sup>4</sup>. It is perfectly consonant with this view that the occurrence of beats is dependent, as in normal hearing, on the objective pitch, *i.e.* on the way in which the basilar membrane is affected by the stimuli, apart from greater sensitivity, attention, etc.

On the other hand the theory of illusion proposed by von Liebermann and Révész is not acceptable. After all an illusion of presentation is only 'illusory' by comparison with the objective stimuli. We cannot suppose that such an illusion belies and covers phenomena as well as realities; for that is what the authors' explanation really means. The impression of correction, they think, is a phenomenon which displaces another phenomenon, which, however, is not phenomenal at the moment. If musical experience can have the associative effects here ascribed to it, why does von Liebermann not hear the correct (imaged) pitch along with the (sensed) pseudotone? We should expect as much from what we know of visual images. And if it be said that tonal images do not mix themselves into tonal sensations, then the whole explanation given

<sup>1</sup> Orthosymphony seems to be better with strong tones. See L. and R. 2, 292, 310. Cf. what is noted above, that intense single tones give no pseudotone.

<sup>2</sup> L. and R. 2, 299.

<sup>3</sup> L. and R. report no gliding tone (*Gleitton*) at the moment in which the pseudotone is heard in the chord by the analytic attention. Nor should we then expect any; on my hypothesis no change takes place on the basilar membrane at this moment. But when the pseudotone changed during a single observation and a *Gleitton* was heard (see L. and R. 2, 306) we may suppose that the basilar membrane actually shifted its point of maximum yield to the stimulus.

<sup>4</sup> A theory of hearing which relies on the statement of the preceding sentence alone must appear to be arbitrary. Why should the effects of all the sub-maximally stimulated resonators be suppressed?



by the authors is invalid. It is therefore best to suppose that in all cases the pitch heard is phenomenally given, not reproduced<sup>1</sup>. This view is supported by the fact that the presence of the pseudotone in a chord seems to depend as much on the intensity of the components as upon attention<sup>2</sup>. So also, of course, the other aspect of tone-volume, or, as von Liebermann and Révész call it, *Höhe*,—is phenomenally given, not reproduced, in spite of the fact that in this unusual case pitch and volume vary independently.

(d) *Pitch the more precise basis of judgment.* All the statements made by these writers about the different relations of pitch (quality) and of volume (*Höhe*) to absolute judgments in terms of tonal names and to judgments in terms of names of intervals may be accepted as important additions to our knowledge. Evidently the preciser basis of the arrangement of tones and of the naming of them and of their differences (intervals) is pitch; but differences of volume also offer a basis for these judgments, although it is less efficient and exact. This agrees entirely with my proposal to class pitch with local sign and position as a kind of 'order,' to class distances and tone intervals in one group of experiences and to refer them to a common foundation in differences in the attribute of order<sup>3</sup>. Our judgments regarding tones and intervals are best when based on this attribute of pitch. But even when the pitch is distorted so as to be unreliable, the same judgments can be got from the volumes of tones by abstraction from the order most prominent in that volume. This may be compared with the fact that we can compare lengths of line as such without any comparison or superposition of their points, although the latter method is much the more precise. We have then to suppose that the ear provides us with a single series of orders; pitch is judged by the most prominent order; interval by the 'form' constituted by the prominent orders; volume by the line or mass of orders stimulated at all; pitches and intervals can therefore be compared and fixed to some extent by means of volumes alone.

(e) *von Liebermann's deep symphonic pseudotone.* The hypothesis I have indicated can also be applied to the facts observed by von Liebermann<sup>4</sup> relating to the raising of a deep tone, e.g.  $C_1$  by a fifth when

<sup>1</sup> "Der orthosymphonische Zusammenhang erscheint, wenn der Korrektioneindruck da ist, normal in jeder Beziehung," L. and R. 2, 299. Then it is normal.

<sup>2</sup> "Auch wenn der Zweiklang unmittelbar nach dem Anschlage orthosymphonisch erscheint, so kommt doch mit fortschreitendem Abklingen der Pseudoton immer mehr zur Geltung," L. and R. 2, 310.

<sup>3</sup> This *Journal*, 1911, iv. 143 ff., 172 ff., 179 f.

<sup>4</sup> Cf. below, 38 ff.

<sup>5</sup> L. and R. 3, 325 ff.



played after its octave  $C$ . The affected region extended from  $E_1$  to  $B_1$ , for only within this area was displacement difficult or impossible<sup>1</sup>. There the objective stimulus and the abnormal sensitivity of the ear coincided. Or else the region  $B_2-D_1\sharp$  was less sensitive than usual. What the physical cause of these peculiar changes of pitch was, it is not easy to imagine. But it is abundantly clear that it was not central, but peripheral. For the phenomenon was not subject to von Liebermann's choice: it was compulsory. The five reasons given in the text<sup>2</sup> for a central basis are: (1) that the true tone appeared on insistence upon it; (2) that the idea of a comparative  $C$  turned  $C_1$  into  $G_1$ , although only  $C_1$  was sounded; (3) that the disposition of  $C_1$  to become  $G_1$  varied from time to time; (4) that the occurrence of  $C_1$  or  $G_1$  sometimes depended upon the will and attention; and (5) that only the pitch of the tone was affected, not the intensity, timbre, or volume. But these reasons are not cogent. For in the main abnormality shown by this subject, which can hardly be supposed to be of 'central' origin, insistent attention also affected the phenomena in orthosymphony, and the disposition to abnormality changed. The second reason given may be supposed to be due to the effect of attention coming to the support of the greater sensitivity of the ear. Without the help of the attention the normal disposition for single tones is stronger than the abnormal<sup>3</sup>. The physical basis of the abnormality may be to some extent concerned with the effect of the momentary intermingling of the stimulations which takes place when a deep tone follows its octave. But it must remain obscure on any theory. As the phenomena are the opposite of orthosymphony, it might be thought to be due to blunting of one region, not to increase of sensitivity of the other— $E_1-B_1$ .

(f) *Binaural mixture*. The hypothesis accepted by me can also be extended to the facts of binaural mixture observed by these writers<sup>4</sup>. It is well known that the ears of many persons, if tested singly, are found to be of different pitch; for the same objective stimulus they render two tones of slightly different pitch. But when both ears are acting at once, no diplacusis occurs<sup>5</sup>. In von Liebermann this pitch difference occurred in a higher degree than usual. Each uniaural tone seemed to stand away from the binaural tone in opposite directions. If the uniaural tones were of equal intensity, the binaural tone was that tone which is normally evoked by the average of the numbers of vibrations which normally evoke such uniaural tones as were heard. If the uniaural

<sup>1</sup> *Op. cit.* 327.<sup>2</sup> *Op. cit.* 331 f.<sup>3</sup> *Op. cit.* 332.<sup>4</sup> L. and R. 4, 676 ff.<sup>5</sup> Cf. Stumpf, *op. cit.* II. 320 ff.

tones were of different intensity, the pitch of the binaural tone approximated correspondingly to the pitch of the stronger tone<sup>1</sup>. Von Liebermann and Révész see in these facts evidence of a mixture of tonal qualities. The comparison is just, provided of course it is correct to classify pitches as tonal qualities. They take pains to explain why tonal mixtures within a single ear do not occur, viz. because uniaural tonal qualities occur in different 'heights.' But if this be a 'reason,' do we not then lose all touch with the analogy of vision? We do indeed; for at the next moment we read that 'heights' are (not, as it were, differences of brightness, as we read elsewhere<sup>2</sup>, but) as it were corresponding points of the two ears; so that when the same 'height' occurs in both ears, the qualities mix. And if these observations are evidence of tonal mixture, then the resulting mid-tone must be supposed to resemble the two mixing tones. Then, as Révész suggests elsewhere, the whole variety of tonal 'qualities'—the scale between octaves—will also be reducible to two end qualities. But it has commonly been denied that *d* resembles *c* and *e*, as an orange-red resembles both red and orange.

*Theory of its occurrence.* A sufficient explanation of these binaural 'mixtures' can be found in a much simpler hypothesis. For it does not involve the admission of uniaural mixtures, and it demands neither the presence of phenomena which do not occur, nor the assumption of phenomenal illusions of presentation. It is agreed that the pitch of a tone is determined by the point of the basilar membrane that is most intensely stimulated. This maximum of stimulation is surrounded on both sides by a region of decreasing intensity of excitation. Now in binaural hearing, as the objective stimulus for both ears is the same, the length of the basilar membrane affected will remain the same, and therefore the volume of the tone in each ear will be the same. We have only to suppose, what is highly probable,—that all the points or pitches of the one basilar membrane are connected with those of the other physiologically, and the phenomena observed by von Liebermann and Révész will immediately follow. For then a superposition of the stimulations, *i.e.* of their intensities, would follow<sup>3</sup>. If the pitch displacement of the two ears is small (in von Liebermann's case it was never greater than a semitone<sup>4</sup>), the resultant order or pitch will

<sup>1</sup> Cf. also Révész, *Zur Grundlegung der Tonpsychologie*, 63. Stumpf noticed something like this, *op. cit.* II. 326 f.      <sup>2</sup> Révész, *Zur Grundlegung der Tonpsychologie*, 41 f.

<sup>3</sup> Cf. L. and R. 4, 680. The binaural tone is stronger than either uniaural tone.

<sup>4</sup> Révész, *Zur Grundlegung der Tonpsychologie*, 64.

naturally fall where there is the greatest resultant stimulation; as the intensity from the other ear increases, the maximum point will move over gradually to the pitch of the other ear. The physiological hypothesis here involved is already familiar in Bernstein's theory<sup>1</sup>. It is, in fact, merely an application of Bernstein's hypothesis to the facts of hearing. But it would not at all follow that a similar mixture should hold for uniaural tones. For there the superposition of intensities is affected by a number of other physical disturbances which give rise to such things as beats<sup>2</sup>. We suppose, that is to say, for uniaural tones that there are really two different stimuli, two rates of vibration, whereas in this binaural case the stimulus remains the same; only the way it affects each ear differs, because each ear differs. It is at least evident that the mixture theory of von Liebermann and Révész does not necessarily apply to the whole series of their qualities at all, and the analogy with vision must thus far break down. Besides their whole theory of the segregation of miscible qualities by means of the attached 'heights' must fall to the ground, unless they can show why 'quality' is segregated with 'height' at all. And the doing of that is excluded by the very use of the category of quality. Similarly Révész cannot show why in von Liebermann certain qualities have dropped out, while others remain, and why different stimuli can excite the sole remaining pitch. Will he recur, like Jaensch, to the assumption of differences of age amongst his qualities? The use of the category of quality blocks all advance here. Nor can Révész explain why certain qualities drop out only in certain octaves. If the lost qualities are really lost, they should be lost altogether.

In certain cases, where perhaps the pitch difference of the two ears is greater and where one ear has been mistuned in an irregular way<sup>3</sup>,

<sup>1</sup> Cf. in W. Nagel's *Handbuch der Physiol. des Menschen*, T. Thunberg, "Physiol. der Druck-, Temp.- u. Schmerzempf." 1905, 720 ff. Cf. A. A. Gray, *op. cit.*, who refers to the discrimination of tactual points for an analogy to the discrimination of tones on the single basilar membrane.

<sup>2</sup> But under certain circumstances a better mixture can be got than results from the simultaneous action of two neighbouring tones, viz. by 'mixing' many neighbouring tones. Cf. S. Baley, *Ztsch. f. Psychol.* 1913, LXVII. 271 ff. It need hardly be said that Baley's results do not favour Helmholtz's theory as against Ewald's or mine.

<sup>3</sup> I have recently had occasion to observe this in myself in degrees varying from a semi-tone of difference to a just noticeable deterioration of the timbre of sounds. In the former case the flat pseudotone is located in or opposite the left ear and it has an unpleasantly metallic timbre; melodies and voices of the appropriate pitch are heard double, but the false ones are devoid of any proper tonality. In the latter case tones that are usually full and round sound slightly flat and rather metallic.



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both tones are heard at once, as if they were presented to a single ear, except that no beats occur<sup>1</sup>. A well trained observer will then be able to attend to the tones of each ear separately. Without this attention the general effect will still be that of a great dissonance, since the superposition of the maxima of the two ears will produce a resultant containing either two maxima or a line of maximal stimulation, *i.e.* the effect produced in one ear when dissonances arise from the simultaneous occurrence of neighbouring tones. At the same time the volumes of the two tones will not quite coincide, whereas in orthosymphony they do, thus giving a degree of fusion that is not consistent with the pseudo-tonal components that appear upon attentive analysis. There should be rather narrow limits to the possibility of cases like that of von Liebermann, where there is an even passage from the maximum of one ear to that of the other. A change in the resonators of one ear, whereby they are lengthened or shortened and so respond maximally to lower or higher tones than usual, forms a sort of auditory squint.

(g) *Vocality*. Révész attempts to show that vocality is a third aspect of sound alongside quality and height<sup>2</sup>. His chief argument is the case of N. N., who could only hear as far as  $e^2$ , and yet could recognise the vowel *a* and even *e*, *i.e.* (according to Jaensch) average rates of vibration of 1000 ( $c^3$ ) and 2000 ( $c^4$ ) per sec. If this observation is correct as it stands, vocality would become not only an aspect of sound, but a component of it, a kind of subsense. The observation must therefore be treated with great reserve. Besides an average rate of vibration with a fair variation might very well be heard when no tone could be heard. In the same way, we hear vowels without being able to sing their pitch, especially when they are given on a fundamental tone, as the vowels were given to N. N. (on  $c^0$ ). A vowel is a complex and irregular impression which might well be effective when a regular impression was ineffective.

(h) *Interval*. Révész has devoted a considerable part of his book—*Zur Grundlegung der Tonpsychologie*—to a discussion of interval. Interval, like the pitch of a tone, may be judged either promptly or by slow laborious effort. The former is based in both cases on differences of quality, or, as I call it, pitch; the other rests upon the vaguer *Höhe* or volume<sup>3</sup>. The former Révész calls interval, the latter distance.

<sup>1</sup> Cf. Stumpf, *op. cit.* II. 460; L. and R. 1, 269, 274.

<sup>2</sup> *Zur Grundlegung der Tonpsychologie*, 84 ff.

<sup>3</sup> In connexion with this distinction Stumpf's note on the possibility of such a thing is of interest. *Op. cit.* II. 336.



There is, as he shows, a great difference between the intervals formed by successive tones and those formed by simultaneous tones. The latter are much harder to analyse; very often the tones have to be attended to successively. Differences of 'height,' *i.e.* distances, recede very much in chords. And even the pitches of tones and the 'intervals' based on them are not easy to hear then. Spatial symbolism, *e.g.* deeper, higher, is only applicable to successive intervals.

*Further theoretical indications.* These and other facts mentioned by Révész are immediately explicable on the hypotheses I would retain for the physiology of hearing. For if pitch depends on the more intensely stimulated region of the basilar membrane, it is obvious that two pitches together will immediately offer great difficulties, especially when both are given equally intensely. They will tend to suppress one another or to detract from one another's 'point.' Only if the attention is turned to them successively, supporting first one and making it more intense and then the other, will the analysis be easy<sup>1</sup>. Orthosymphony, even on the illusional theory given by von Liebermann and Révész, may itself be partly accounted for by this means, although the fact that the two tones cooperated and so stimulated the basilar membrane quite as they normally do, is of primary importance. In chords, moreover, the extents of the basilar membrane excited by the components of the total sound wave must often be largely coincident, so that there must be a large amount of identity amongst the orders of the elements of auditory sense of which they consist, and they will therefore in many cases fuse very well. The more exact the coincidence of the parts of the basilar membrane that are stimulated, the more will the maximal region of the higher tone add itself evenly and regularly to the maximal region of the lower, extending it and it may be giving it a slightly different form. Thus tones which fuse according to volume will tend to fuse according to pitch and two tones will then tend to be taken for one. Only when the maximal points of the fusing tones are close together, will they interfere with each other markedly and give a broad line of maximal stimulation with oscillations due to the differences in the number of vibrations per second. They will be heard as increasing dissonances, the nearer they are together and the nearer they come to an even superposition without reaching that entirely. Some chords will therefore be easy to analyse, others will be difficult. But whatever the ease or difficulty of analysis may be, each chord will be a characteristic complex of tonal orders and

<sup>1</sup> Cf. Stumpf's "Mit dem Ohre singen," *op. cit.* II. 291 ff.

so will be recognisable as a unitary whole, or at least distinguishable from other complexes, whatever the stage may be at which the analytical training of the individual has reached. The same hypothesis offers an explanation for the fact that musical people often fail to notice the presence of difference-tones and overtones. They notice generally only what their attention is trained to pick out by the successive analysis of the attention. All the rest goes into the sound picture; it does not remain ineffective; it makes a difference to the total pitch picture; but it does not excite analysis until the attention is turned by special means towards the difference-tones and partials<sup>1</sup>. The facts of the separate discovery of difference-tones, combination-tones, and overtones is an obvious proof of this<sup>2</sup>.

This view also explains why in a complex tone or in a single chord the fundamental tone is the most easily apprehended. It forms the greatest extent of stimulation, its maximum is more widely spread, and also more intense as a maximum than that of any other tone in the chord, unless it be a much higher and very intense tone. When successive chords, however, are given as accompaniment to a melody, it is the melody which is noticed most easily, *i.e.* oftenest by musical and unmusical observers alike. For the melody is commonly the part that moves most connectedly and most effectively<sup>3</sup>. This motion draws the attention most to itself, just as it does in vision, where also of stationary points of light the most intense is most noticeable.

As regards the ambiguity of intervals formed by 'qualities' divorced from their usual 'height,' the facts recounted by Révész<sup>4</sup> only confirm the distinction between 'quality' (pitch) and 'height' (volume). Any and every theory of these facts has still to find a reason why the ambiguity of an interval given by hypothetically pure and unattached

<sup>1</sup> Cf. Stumpf, *op. cit.* II. 232.

<sup>2</sup> It must be evident that since the fusion of tones is not a property of the elementary sensation, but only of complexes of two or more sensations, we must give a psychological theory of it first (cf. Stumpf, *op. cit.* II. 211 ff.) and use that to formulate a physiological basis for it. Both these kinds of theory, of course, will refer primarily to the corresponding properties of the most elementary auditory sensations we actually get. These, however, must be analysed into complexes of hypothetically ultimate elements of auditory sensation, which differ from those of other senses only in the matter of quality. The structure of the complexes which form our simplest auditory sensations, must explain all the special peculiarities of sound.

<sup>3</sup> Cf. Stumpf, *op. cit.* II. 337 ff.; also 393, "Bei aufeinanderfolgenden Zusammenklängen macht das Ganze scheinbar die Bewegung der in den grössten Schritten bewegten Stimme mit."

<sup>4</sup> *Zur Grundlegung der Tonpsychologie*, 113 ff.

'qualities,' e.g.  $c-e$ , was perhaps oftenest determined to that of a rising third, why it was sometimes unresolved and therefore arbitrary, and why it was sometimes determined to that of a falling minor sixth. Révész's facts nowhere prove the absolute independence of 'quality' and 'height,' but only the relative independence of them<sup>1</sup>. Nor can he raise more than a presumption in favour of identifying the 'qualities' of a tone and its octave; for in spite of their similarity and approximation to identity, there can be no doubt that the uppermost quality of an octave comes next after that of the tone that just precedes it, when we play up the scale; it does not leave the latter aside to begin afresh, as we should have to admit, if we suppose with Révész that the tone  $c$  and the tone just less than  $c^1$  are maximal differences from which all intervening tones are probably derived by mixture. Besides, Révész's theory of the absolute independence and ambiguity of quality does not do justice to the fact that we can begin an octave on any quality whatsoever. Also if 'qualities' were miscible, we should for that reason have to suppose that the maximal differences within an octave were all those qualities which are separated by a little more than the tritone interval<sup>2</sup>.

Moreover the facts suffice in no way to show, as Révész maintains<sup>3</sup>, that an interval has a direction only because it is normally, i.e. habitually, associated with a difference of 'height.' Here we encounter again the misleading influence of the analogy with vision. If 'height' has direction, it must also include some basis of 'position'; by what means then is Révész going to show how his 'qualities' have got linked up normally with differences of position? And if he attributes some rudiment of position to his 'qualities' also, he will find it hard to show why there is only one range of them, while there is a much larger range of 'heights' and therefore of positional differences, capable of using up the octave over and over again. Of course there is a problem involved in the facts. But that problem is to explain the special differentiation of the ends of the series of pitches, not by any means to explain that the ends are distinguished at all and that there are two directions of change within the series. Positions and directions are distinguished in touch, vision, and articular sensation; the last of these forms the best analogy to sound, as it presents so often a single series. But in sound we call the one end high and the direction towards that end 'rising,' the other end

<sup>1</sup> Or, more strictly, of volume and the most pronounced 'order' within that volume.

<sup>2</sup> Cf. Stumpf, *op. cit.* II. 201 ff.; Révész, *Zur Grundlegung der Tonpsychologie*, 134.

<sup>3</sup> *Tonpsychologie*, 116 ff.



low and falling. This special distinction, I take it, is one that could well be explained by the help of the normal accompaniment of differences of volume. For these latter differences form a series with obviously different ends, the one having first a physiological, and then a physical limit<sup>1</sup> of smallness, the other hardly a limit of largeness. The occasional ambiguity of the direction of intervals is more easily explained by the peculiar conflict they entered into in von Liebermann's case with the direction of volume-differences. Other cases may obviously be put down to the confusion of scale relations, *e.g.* thirds, tenths, etc. Besides, of course, Révész's appeal to habit belies the very kind of discovery he derives from von Liebermann's observations, viz. that the 'habit' of the normal correlation of 'quality' and 'height' can be dissolved and observed in dissolution.

After all, the conviction must force itself upon one that the octave relationship is not to be explained by a presumption of identity based on the presence of great similarity, but by reference to the fundamental properties of all sensation in relation to the simplest complexes in which auditory sensations are given to us, *i.e.* by relation to coincidences of volume. It is only such a theory that can explain the absolute relativity of scale relationships, which is, nevertheless, accompanied throughout a large part of the auditory range by the discrimination of practically absolutely equal differences of pitch. The decrease of clearness of pitch at the lower end may be readily explained by the greater diffusion of the region of maximal stimulation, and the analogous nature of the upper limit by the difficulty the soft basilar membrane presents towards fine stimulatory differentiation as well as the want of a sufficient number of sensitive spots to receive the fine differences of position which successive tones imply. But as the volumes are extents, there might still be a fair differentiation of tones in that respect; although it is obvious that both pitch and volume differences must deteriorate together in the highest regions. Still as the extent has always a greater basis than pitch, the former might somewhat outlast the latter in high tones<sup>2</sup>.

As for all the problems, "die gar nichts Problematisches haben, so lange man Intervall mit Distanz identifiziert," namely the problems of transposition and inversion, it must now be clear that they arise only

<sup>1</sup> Thus I dissent from the view of Révész and others that the *Höhenreihe* is 'prinzipiell unendlich,' *Zur Grundlegung der Tonpsychologie*, 87.

<sup>2</sup> Cf. Stumpf, *op. cit.* II. 57, "Wo man noch den bestimmten Eindruck hat, dass ein Ton spitzer ist als ein anderer, den man doch seiner Qualität nach nicht mehr davon unterscheiden würde."



for those who identify pitch with quality and volume with something quite heterogeneous—height. If the relation between pitch and volume I have suggested is adopted, it becomes clear at once that transposition and inversion and the like are only matters of form, which present no other problem than do any matters of form, *e.g.* those of vision and touch. We recognise a form more or less in any part of the visual field and more or less in any relation to the frontal plane, *e.g.* laid on its side, or turned upside down. And hearing offers us parallels to practically all the familiar processes of change of form, namely motion<sup>1</sup>, speed and all their relations to emotional life as signs of the activity of experience in general.

#### V. *The nature of the system of hearing.*

The result of the preceding discussion of later researches on hearing is to confirm on all notable issues the formula I proposed for auditory sensation on the basis of a general study of the common attributes of sensation. I need not again proclaim any of the principles upon which I found a study of the senses that is both systematic and special. In fact they must occur to anyone who sets himself the task of such a study, confident that it must succeed.

We must therefore admit that the sense of hearing brings us only one quality, differences of intensity, of pitch (which falls under the generic head of 'order'), of volume, and of temporal attributes. These differences of volume introduce the problem of compound sensations and we solve that problem by postulating primary atoms of audition, which we practically never experience in isolation. We get them only in masses, in which a small region of orders is more or less definitely emphasized by means of intensity. From this psychological analysis and theory we can proceed to formulate the hypothetical nature of parallel physiological processes and to explain all the special phenomena of sound. Only known and commonly accepted physiological hypotheses need hereby be invoked. The psychological construction, on the contrary, is largely new; but that is no sign of its error, but only of the failure of previous efforts to find the correct line of analysis. Nothing else was wanting; for the psychological hypotheses involved in it are in other spheres of sensation the most trivial and familiar. No one could doubt that areal experiences of colour are obtained physiologically by the simultaneous stimulation of a large number of neighbouring sense-

<sup>1</sup> Cf. *This Journal*, 1911, iv. 169 ff.

organs and psychologically by the fusion of a large number of elementary (*i.e.* smallest known to us) sensations of colour, differing similarly in order from one another and fusing in virtue of their extensity to an area. We have only to add the hypothesis that in hearing we never get sensations more elementary than such areas or extents. To turn from the clear daylight of such a view to the darkness of qualitative differences is to nourish a passion for ignorance and scepticism. On the qualitative line of analysis the time when we shall reach some understanding of the sense of hearing is indeed far distant.

Only one point need again be emphasized<sup>1</sup>. The failure to appreciate it has been a common barrier to progress. Pitch and volume constantly urge their true nature upon our attention, but it has been called 'quasi-spatial' and so the truth has been ignored and suppressed<sup>2</sup>. But in fact far from being quasi-spatial, they are non-spatial. They are simply systemic, *i.e.* such orders and such continuousness as will with sufficient variation of order constitute a sensory system. The practically most important correlation of differences of systemic order in most senses happens to link them to the spatial differences of material things, both really and cognitively, both as a matter of fact and as a matter for our knowledge. So we call the tactual, the visual and other systems spatial. In the sense of hearing order differences are linked to spatial differences of matter only in fact; cognitively they are not so. But they form none the less a continuous system of positions. If we talk of this system as quasi-spatial, we should talk not only of the perceptual, but also of our conceptual systems as quasi-spatial. But that would be quite misleading. They are no more necessarily cognitively spatial than is any system of numbers. They are simply ordinal. Temporal differences of position are also ordinal. I distinguish this order from the other by calling the former temporal and the latter systemic<sup>3</sup>.

#### VI. *Physiological theory of hearing.*

A physiological theory of hearing must fulfil three sets of requirements: (1) it must account for all the observed facts of hearing, and it must correspond adequately to the result of the general analysis and systematisation of auditory experiences; (2) it must be compatible with what is known or probable regarding the physical nature of the basilar membrane, etc.; (3) it must show how the sense of hearing has, or

<sup>1</sup> Cf. *This Journal*, 1911, iv. 142.

<sup>2</sup> Cf. Stumpf, *op. cit.* II. 55 ff., 58 f., etc.

<sup>3</sup> Cf. *This Journal*, 1913, vi. 241.

could have, developed, both physically and psychically, from its lower forms to the relatively advanced form in which we find it. Of the chief theories of hearing, Helmholtz's satisfies the first two of these conditions as well as might be, but as commonly understood, it does not satisfy the third at all. The general distribution of the functions of hearing over the basilar membrane which it assumes, appears to be the correct one, and it also correctly admits only one (at least only one most intensely stimulated) resonator for each tonal sensation. Ewald's theory, on the contrary, posits a whole row of similar physical processes for each single tonal sensation, and finds great difficulty in conceiving such connexions between the parts of the basilar membrane involved in the various sound-pictures and the 'centres' for each tone as will afford our actual awareness of tones. Ewald's theory is thus untrue to experience; we ought to hear for each sound picture a series of identical tones, but we do not. And it is as impossible to imagine how the connexions Ewald postulates, can have developed, as it is to imagine how a series of resonators can be placed in the ear and how the different 'qualities' (pitches) of tones came to be linked to them properly on the common psychological interpretation of Helmholtz's theory<sup>1</sup>. I wish now to indicate the kind of theory that would combine in it the advantages of the chief theories of hearing hitherto advanced and avoid the errors into which they have fallen. That is just what a theory that is guided by the correct lines of psychological analysis should be able to do. These lines have been found by the guidance of a properly planned analysis of the psychological facts of hearing and this again is inspired by a general consideration of the ways and means of attaining a complete systematisation of all sensory experience, at least of its simpler forms<sup>2</sup>.

<sup>1</sup> Cf. This *Journal*, 1913, vi. 254; also J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, lxxvi. 181 ff.

<sup>2</sup> Even so predominantly physiological a theory as that of Helmholtz has been able, as it ought, to suggest the question, whether "die Töne uns auch flächenhaft ausgebreitet und angeordnet im Bewusstsein erscheinen" (Stumpf, *op. cit.* ii. 101). It is interesting to read Stumpf's note to these words, in which he reports Waitz's objections to supposing that tones are isolated by special organs in the ear. Stumpf says: "Derselbe Punkt schien mir früher bedenklich; doch könnte man, meinte ich, vielleicht noch die Hilfsannahme versuchen, dass die Töne zwar wirklich einen verschiedenen Ort in der Empfindung hätten, aber jeder immer denselben, wodurch der Tonraum ebenso bedeutungslos für unser Bewusstsein würde, als wenn er gar nicht existierte," etc. It can only be the confusing notion that *Ausbreitung* and *Anordnung* must always be spatial, instead of space being a kind of *Ausbreitung* and *Anordnung*, that thus forces Stumpf to make an hypothesis to conceal what is surely present.



As Helmholtz supposes, different parts of the basilar membrane subserve the reception of different tones; the higher whorls (towards the apex) of the cochlea give low tones and the tone evoked becomes the higher, the further we pass towards the basis of the cochlea and towards the root of the basilar membrane in the wall of the sacculæ. It is utterly inconceivable that, as Ewald's experiment suggests, the whole basilar membrane should be involved in the reception of every tone. Ewald's experiment is certainly very ingenious and instructive. But its main value is the proof it gives that the small fraction of his membrane<sup>1</sup> that forms the unit of any sound picture is the actual and sufficient physical response to an enormously greater wave-length. Any other value the experiment might have had, is annulled by its result—the sound picture—and by the fact that the artificial membrane used was straight and of equal breadth along its whole length<sup>2</sup>. Ewald seems to have felt it necessary to modify his theory to meet these considerations; for he suggests that under normal conditions the whole of the basilar membrane would be used only for low tones and that the higher a tone, the more it would confine itself to the narrower end of the membrane<sup>3</sup>. As regards the tone-picture, however few the number of repetitions it may contain, Ewald seems also to have felt the force of the psychological objection, that we do not for each rate of vibration hear a row of identical tones corresponding to the number of standing waves; for he sketches an hypothesis<sup>4</sup> which shall circumvent the necessity for this conclusion. But such an hypothesis is developmentally more impossible than is the correlation of a row of resonators with a row of pitches or 'qualities' on Helmholtz's view. Ewald himself points out this weakness in Helmholtz's theory<sup>5</sup>.

The psychological analysis of hearing postulates not only that different parts of the basilar membrane shall subserve the reception of different tones, but that only one unitary part of it shall subserve the reception of each regular rate of vibration. This postulate is confirmed by the result of ter Kuile's investigation, who holds that for tones of different pitch a variable length of the basilar membrane is bulged out, beginning from the base and proceeding towards the apex of the cochlea. This view would give a good physiological basis for differences of volume, but it is hardly satisfactory in its explanation of pitch; for receptors

<sup>1</sup> Cf. *Arch. f. d. ges. Physiol.* 1903, xciii. 489 f.

<sup>2</sup> Cf. A. A. Gray, *op. cit.* p. 16 of offprint.

<sup>3</sup> *Op. cit.* 1899, lxxvi. 184 f.

<sup>4</sup> *Loc. cit.* lxxvi. 156 ff., 183 ff.

<sup>5</sup> *Loc. cit.* 155.

that stand at the limiting points of the extent stimulated<sup>1</sup>, cannot well be stimulated sufficiently for the purposes of the sharp determination of sensory differences. In sound, as in vision and touch, there will hardly be at the edges of a stimulated extent or area the most rapid change of stimulation, from positive to nothing, or from positive to negative; the change should be gradual in sound. For that reason it seems better to transfer the points of stimulation with Helmholtz to the middle of the extent of stimulation, which decreases in both directions towards the sacculus and towards the hamulus<sup>2</sup>. And if ter Kuile's conception, according to which any tone excites twice as long an extent of the basilar membrane<sup>3</sup> (always starting from its base at the sacculle) as does the octave of that tone, could be combined with Helmholtz's, according to which a certain series of resonators is stimulated for any tone, the amplitude of resonance rising from *nil* at two points towards a maximum between them<sup>4</sup>, it seems to me we should then have a perfectly sufficient basis for a full physical parallel to all the phenomena of sound. To the minimal complexes of auditory sensation which we experience, we should have corresponding minimal physical complexes. Multiplication of these complexes would give on both sides similar secondary features of tone. Fusion, especially, would receive an explanation on this view, as it gets on no other except ter Kuile's and Ewald's. In ter Kuile's theory, however, the task to be fulfilled by the explanation is overdone; for it becomes impossible to see why any but tones of great pitch differences should be distinguishable at all.

I have not yet found in the literature any explanation of the peculiar changes of curvature that are seen in the basilar membrane from its basis to the apex of the cochlea. These cannot be fortuitous, for they are almost exactly the same in all animals, no matter what degree of development their cochlea may present<sup>5</sup>.

High tones would then be evoked by stimulation of the bases of the basilar membrane, while decrease of pitch would carry the extent of the basilar membrane stimulated further and further towards the apex of

<sup>1</sup> *Arch. f. d. ges. Physiol.* 1900, LXXIX. 500 ff. It should also be noted that this limiting point is supposed to move forwards over the extent stimulated once in each tonal period. This oscillation of pitch is hardly reconcilable with the phenomena of hearing.

<sup>2</sup> It is quite consistent with this that the injury caused to the basilar membrane by a long continued intense tone affects not a point, but a short region of it.

<sup>3</sup> *Op. cit.* 201.

<sup>4</sup> This also agrees with Ewald's theory, in so far as a single unit of a tone picture is concerned.

<sup>5</sup> Cf. A. Gray, *The Labyrinth of Animals*, London, 1907, I.; 1908, II.

the cochlea. Lowest tones would probably affect the whole extent of the basilar membrane. But the region of the basilar membrane concerned in the determination of the pitch of the lowest tones would lie approximately at the centre of this extent. It would agree with this view that, as Ewald implies<sup>1</sup>, dogs become deaf to lowest tones only when the upper end of the basilar membrane is considerably shortened. If the hearing of low tones is to be affected by mutilation of the basal end of the membrane, the whole of the first whorl must be destroyed<sup>2</sup>. It is a familiar fact that the differential threshold of pitch does not conform to Weber's law, any more than does the discrimination of points on the skin by the method of successive stimulation. The necessary 'increment' increases slowly with the rise of pitch. I have not been able to ascertain whether the nerve supply to the organs on the basilar membrane decreases from the basis towards the middle or is regularly distributed. Probably the silence of Retzius and other authorities on this point is in favour of the presumption of regularity of distribution. In any case we must suppose that the size of the differential increment of pitch is determined by the nature of the spread of excitation round the maximum and the influence of this upon the displacement necessary to give a noticeable difference. The displacement would be about the same relative amount throughout the scale, the same fraction of a vibration per second. Otherwise we should expect the discrimination of pitch to increase proportionately to the extent of the basilar membrane affected by a tone. Stumpf thinks<sup>3</sup> that the distance, as distinguished from the interval, between two tones increases up to the third accented octave. The nerve supply may possibly be richer near the bases of the cochlea than in the upper whorls; or our notion of the distance between tones may be affected by the number of differences we can distinguish between two bounding tones. Apart from the standardisation of distances which is given by consonance and by the relations of form between tones upon which consonance rests, it seems possible that we should take the finer subdivision we get in the higher octaves for greater distance. We do so in using the fingers or the tongue to appreciate distance; the former are to some extent standardised with other and less discriminating parts of the skin and with vision, but the tongue can hardly be said to be so.

<sup>1</sup> *Arch. f. d. ges. Physiol.* 1899, LXXVI. 179.

<sup>2</sup> It is quite possible that the remainder might after a time develop a response to high tones. Cf. Ewald, *loc. cit.*

<sup>3</sup> *Op. cit.* II. 405.



A physiological theory of hearing of this kind would easily satisfy all the demands of developmental theory. It would do this so far as the development of the receiving membrane is concerned, as easily as does Ewald's theory, and it would do so for the relations of the receiving membrane to the sensory centre, as Ewald's never could. For no new and inexplicable attribute of auditory sensation enters to upset the derivation it makes possible. We need only postulate a membrane of texture and length just sufficient to receive auditory stimulations, no matter how imperfectly, and connected with this a primitive auditory receptor, evoking a sensation of a certain quality<sup>1</sup> of variable intensity, having a certain 'order' aspect and extensity. A multiplication of these elements and an accompanying extension of the membrane will mean a variation of 'order' and a fusion of these orders in virtue of their common extensity or continuitiveness. This multiplication is already admitted in other senses and its biological advantage would be immediately patent in the modification of experience which would result from it alone. The sense of tone would, therefore, as Stumpf suggests<sup>2</sup>, develop from above (high pitches) downwards (to lower pitches). Hand in hand with this development by multiplication would go a development by refinement of texture of the membrane and of the receptors, so that the experience would gradually approximate to that of pure tone, as the lengths of the radial fibres, etc., were systematically adjusted to one another in virtue of the advantages which variation towards that system would make patent through experience. The whole development can be seen at a glance from this point of view<sup>3</sup>. And there is not the least disparity between the peripheral and the central processes of complication or between both of these and the complication of the accompanying experience.

Another advantage of this theory is the ready explanation it offers of the *general nature of hearing*; complex sounds of all kinds first appear blended into a unity<sup>4</sup>, which may afterwards be analysed. But this analysis never approximates to the kind of separation we find between different patches of colour in the visual field. That we can only explain by supposing that the cause of fusion is physiological and is not

<sup>1</sup> Ewald admits that on his theory there is only one "specific energy in Mach's sense" in hearing. *Arch. f. d. ges. Physiol.* 1899, LXXVI. 181.

<sup>2</sup> *Op. cit.* i. 339 ff., ii. 218.

<sup>3</sup> For indications of the process of standardisation, whereby intervals, including the octave, are standardised throughout a large part of the musical scale and whereby pitches come to possess an 'absolute' identity and name, see below, 40 ff.

<sup>4</sup> Cf. Stumpf, *op. cit.* ii. 77.

removable<sup>1</sup>. I suppose that the extent of the basilar membrane stimulated by every tone begins at the base near the oval window and extends towards the apex of the cochlea. The extent of basilar membrane involved by a higher tone will therefore always coincide with a part of that involved by a lower tone. Only in the case of a large difference of pitch will the maximal point of the higher tone stand well away from that of the lower and so be readily noticeable.

We should expect to be able to deduce the relative degrees of *fusion* shown in different intervals from a consideration of the nature of the supposed coincidence of extents. Thus it is evident that the extent involved by the octave of a tone will be the part reaching from the base of the basilar membrane to the maximal point of the lower tone. In the interval of the fifth the maximal point of stimulation of the higher tone and the end-point of the extent of the basilar membrane it involves will severally lie on either side of the maximal point of the lower tone and away from it by one-sixth of the whole extent involved by this lower tone. The corresponding relations for the fourth are one-eighth towards the base, one-quarter towards the apex. The next degree of fusion applies, according to Stumpf<sup>2</sup>, to four intervals (4:5, 5:6, 3:5, 5:8); the corresponding relations for these are: one-tenth and three-tenths, one-twelfth and one-third, one-fifth and one-tenth, three-sixteenths and one-eighth. The relations for the tritone are three-fourteenths and one-fourteenth, and for the second and seventh one-eighteenth and seven-eighteenths, seven-thirtieths and one-thirtieth. Thus we see that only for the octave and the fifth is there complete balance in relation to the maximal point of the lower tone. For the fourth the balance is uneven, but it is only as two to one, and the denominators are still small. For all other intervals the denominators are larger and the balance is usually less. These facts seem to claim some significance alongside the oscillations of the stimulations which must accompany dissonances.

Beyond the interval of the octave, however, these relations must cease to be of much significance. And there is no doubt that fusion is then less<sup>3</sup>, although the same harmonic affinities are maintained by indirect means. I mean by that, not overtones or other secondary accompaniments of chords, but the system of tonality that has grown up on the basis of the greatest and primary fusions.

<sup>1</sup> *Op. cit.* II, 211 ff.

<sup>2</sup> *Op. cit.* II, 135; cf. W. Kemp, *Arch. f. d. ges. Psychol.* 1913, XXIX, 162 f.

<sup>3</sup> Cf. Kemp, *op. cit.* 162 f.

It is most natural that the grades of *sensuous pleasantness* of intervals should differ greatly from their grades of fusion<sup>1</sup>. For the former we have to appeal to other features of the tonal complex than its fusional aspect, namely its variety, a certain tension of parts not reaching as far as discord and the suitable footing for subjective activity that arises therefrom. But I shall not attempt to construct the physiological or psychological basis of all the phenomena of fusion. That is a task for special investigation. I wish merely to indicate the lines my general analysis would extend to meet the facts. I can see in the facts nothing which would constitute a serious objection to my analysis. On the contrary the facts rather seem to invite the application of it.

The relation of *attention* to analysis also receives a place in my theory. As Stumpf says: "Several simultaneous tones can be sensed and rough differences between them can be noticed without ado; finer differences only after practice and other favourable circumstances<sup>2</sup>." The aid given to attention by sounding one or more of the components of a chord before the whole chord, consists in the fact that the attention is thereby directed to that particular point in the tonal order which is to appear in the chord; the tone heard is thereby intensified, for its entrance is made easier and quicker. As the attention passes from one component to another, each is intensified and heard specially<sup>3</sup>. But we can only thus intensify what is already a relative maximum in relation to the just surrounding degrees of excitation. We cannot, as Stumpf<sup>4</sup> thinks we should be able to do on Helmholtz's view regarding the spread of sympathetic resonance on the basilar membrane, intensify by the attention any less than maximal points that are in no proper sense relative maxima. At the same time this does not prejudice the possibility that the pitch of low tones, for which the relative increase of resonance up to the maximum is very gradual, should be rather indeterminate.

The attention cannot without effort attend to all the differences that are given<sup>5</sup>; it must be trained, just as for the perception of the niceties of visual form; and it can be trained in different directions,—for analysis of the objective components of chords, for overtones and difference-tones, for accompanying significant noises, etc.; and the effect of imperfect training may fade out in time. Yet whatever the degree

<sup>1</sup> *Op. cit.* 192 f., 249 ff.

<sup>2</sup> *Op. cit.* II. 85.

<sup>3</sup> Cf. Stumpf, *op. cit.* II. 314, "dass mit dem gleichzeitigen Heraushören (zweier Obertöne) nicht, wie mit dem einzelner Obertöne eine Verstärkung verbunden ist. Verstärken kann man immer nur einen auf einmal."

<sup>4</sup> *Op. cit.* II. 113 f.

<sup>5</sup> For conditions affecting analysis see Stumpf, *op. cit.* II. 328 ff.



of training may be, all given differences are there and may be familiar in bulk without any analysis. Thus persons who make no analysis of sounds so as to single out their components one by one, may still recognise voices, noises, vowels, etc., simply by a comparison and distinction of these sounds in bulk. Limits may be set to analysis in any person by the nature of his basilar membranes; only a fine membrane will make very pure tones and a high power of analysis possible; in a coarser membrane total sound complexes, though tending in character more towards noise, will still be distinguishable and a certain amount of analysis will be easily attained. For noises are less constant and precise stimulations. So the powers of the musical and of the unmusical are all of the same nature and origin. Unmusical people are compelled to recognise sounds by a 'hear and say' method, as it were; those who have absolute ear may recognise most chords also in this way, while they will still spell out unfamiliar ones.

The greater ease with which sounds rich in *overtones* can be recognised<sup>1</sup> is just an instance of the greater ease of recognition of the complex as against the simple whole; it is also acquired on the 'hear and say' method. It is not through making the fundamental more precise that the overtones make the fixation of the pitch of the fundamental more precise. But the whole sound complex is made more precise and more easily recognisable by the addition of overtones. For the number of distinguishable differences in an octave increases greatly, as we pass from one octave to the next higher (up to a certain point); these finer differences must, therefore, react through the sound complex as a whole upon the distinction of that complex, when it is treated, as it usually is, as equivalent to the fundamental tone it contains.

But analysis can be carried only up to a certain point; it can never separate a tone from a complex in which it is given, so as to make it in all respects appear exactly as it does when it is given alone. Only in the matter of pitch, apart from quality and the temporal attributes which may be neglected, is there true equivalence. The pitch of the component, except perhaps in the most special cases<sup>2</sup>, is identical with that of the isolated tone. The seeming perfection of tonal analysis rests entirely upon this fact. Intensity and volume, on the contrary, must be very much affected. There can be no true analysis of the intensity<sup>3</sup> of a component, except on the basis of the difference between

<sup>1</sup> Cf. Stumpf, *op. cit.* II. 351.

<sup>2</sup> *Op. cit.* 397 ff. These, however, are not deemed true cases of change of pitch.

<sup>3</sup> Cf. Stumpf, *op. cit.* II. 420, "Die gleichzeitigen Tonempfindungen oder besser die gleichzeitigen Erregungen des Nervus acusticus tun sich gegenseitig einen Abbruch."

the maximum, to which it owes its distinction, and the surrounding level, in so far as that is more or less appreciable. Analysis of volumes, such as would give the definition of volume found in the isolated tone, is also impossible. Component volumes can be detected only unitarily and, as we have supposed, through the secondary aspect of fusion. This is a general effect; analysis may be necessary to ascertain the nature of particular fusions in a complex chord; but we do not first, or at all, separate  $x$  and  $y$  from one another in respect of volume in order to notice their fusion. We attend to the  $x-y$  part of the complex in order to attend best to the  $x-y$  fusion, and that may be helped, of course, by first attending to the pitches  $x$  and  $y$ , so as to direct attention better to the  $x-y$  part of the fusion<sup>1</sup>. It is also obvious that in the process of analysis the unity of the timbre of a component will be largely sacrificed; the component will be heard without its timbre, unless special circumstances, such as the unitary movement of a timbre provided by the concert of several different musical instruments, make it specially easy to attend to a unit of timbre<sup>2</sup>.

Special conditions will attach to the way in which either of two tones covers or obliterates the other, when both are given together. Stumpf's conclusion<sup>3</sup> "that the higher tone must possess a greater excess of intensity if it is to cover the lower one than conversely," seems to be deducible from consideration of the extents of the basilar membrane involved. For the extent of the higher tone will always fall within that of the lower, so that the amplitude of oscillation it produces will add itself to that of the lower tone. If the higher is to suppress the lower its maximum must lie so near to that of the lower and its intensity must be so strong that, when it is added to that of the lower, the relative increase of the maximum of the latter over its neighbours will no longer be enough to give a noticeable difference. The lower tone, on the contrary, has the advantage of being the only clearly defined one; it begins at the base of the basilar membrane and ends clear of all the higher tones. The others only help to colour it, to modify its form, so that it naturally usurps to itself the chief attention. If a higher tone is to be heard, it must not only make a bigger relative maximum (building already on a considerable amplitude) than it would in isolation; but it must be strong enough to call the attention away

<sup>1</sup> Cf. Kemp, *op. cit.* 214 ff., Summary, 235.

<sup>2</sup> For observations of the condition of tones in more or less stationary complexes, cf. W. Köhler, *Ztsch. f. Psychol.* 1913, LXIV. 100 ff.

<sup>3</sup> *Op. cit.* II. 228, cf. also from 219 onwards.

from the lower tone. This predominance of the lower tone in unchanging tonal complexes is probably the natural basis of Külpe's law of consonance, "dass bei gleichen Verschmelzungsgrade der einen Akkord zusammensetzenden Intervalle der grössere Verschmelzungsgrad des am tiefsten liegenden Intervalls für die höhere Verschmelzung des Ganzen den Ausschlag giebt<sup>1</sup>." That the special fusional relations of tones will modify these general rules is obvious. But fusional relations, as I have supposed, are to be considered merely as special cases of these rules<sup>2</sup>. "In a resting chord the whole seems to have the pitch of the deepest tone, even when this is not also the strongest<sup>3</sup>."

An important deduction regarding the facts of pathological hearing can also be made. The excitation of the basilar membrane that is produced by a tone that is the octave of another tone, just reaches the point of maximal stimulation of the latter tone. Any tone higher than the octave will not reach this maximal point and so will not include it within its extent. On the other hand the excitation of every tone lower than a given tone will include the maximal point of the latter within its extent. Now we may suppose that every diplacusis or pseudotone that is due to an affection of the basilar membrane arises because within the total extent excited by a tone there is, besides the point of maximal objective stimulation which gives the pitch of the objective tone, a sensitive point that is for some reason more sensitive than usual and so gives a pseudotone. It would then follow that the objective tone may be, according to circumstances and cases, any tone that is lower than the pseudotone, but never more than an octave higher than the pseudotone, if indeed it ever quite reaches the octave<sup>4</sup>.

It is now easy to apply this theory of hearing to the exceptional facts gathered by von Liebermann and Révész. In a certain sense there is contained in any normal tone every pseudotone that can ever take its place, so long as the physical stimulus remains the same<sup>5</sup>. Pseudotones, it is clear, may be caused by any such alteration of the basilar membrane as changes the point of maximal stimulation, whether this process represent an increase of sensitivity somewhere, or a loss of sensitivity somewhere, or both, or a more or less regular change in the physical nature of the resonators of the basilar membrane. The pseudo-pitch is

<sup>1</sup> See Kemp, *op. cit.* 207.

<sup>2</sup> Cf. Stumpf, *op. cit.* II. 234.

<sup>3</sup> Stumpf, *op. cit.* II. 384, 407.

<sup>4</sup> According to Stumpf, *op. cit.* I. 277, mistuning in diplacusis to more than one-third probably rests upon illusion.

<sup>5</sup> Cf. above, p. 15.



not really, but merely phenomenally, new; it is always contained in the normal tone, but it is then strictly subordinated to the normally most prominent pitch, *each tone being a regular system of sounds*. Noises are also sounds, but they are not the kind of system of sound that constitutes tone; they are not regular in system, but irregular; the irregularity may be of many kinds, just as noises are. We can hardly expect to be able to classify noises in any exhaustive manner. My analysis shows that it is scarcely a matter of great interest to do so. Theoretical interest centres chiefly on the question of the pitch of noises. Their pitch, however definite it be, will always differ from that of a tone by reason of the absence of the tonal system in which the pitch stands. But the very irregularity that surrounds the pitch must help to hide it. It will not predominate in a system of pitches, but it will merely be one of many simultaneous, or very rapidly changing pitches. Some degree of determination of the bulk of these will always be possible; we shall hear whether it is a low or a high sound, and in each case approximately how low or high. No more is surely needed. We do not ask the theorist who discusses the localisation of visual sensation to find the local sign of the sunbeam dancing on the water.

With regard to the appreciation of intervals, the recent results obtained by Catharina v. Maltzew<sup>1</sup> are of importance. She has shown (1) that the judgment of successive intervals is not based upon their conversion into fused simultaneous intervals; the judgment is not based upon that form of the fusional relationships of tones. Nor is it (2) based upon 'distance'; for, as Stumpf observed, distance and interval do not vary together<sup>2</sup>; descending intervals are harder to judge than ascending, whereas both are alike in the matter of distance; and the introspections of Fr. v. Maltzew's observers show that the 'distance' between tones is a very unreliable basis of judgment, unless perhaps for grosser differences. She points out further (3) that 'distances' are gradually variable quantities, whereas interval varies qualitatively. The fact that in successive intervals we pass from one tone to another, leads her to recognise a special qualitatively peculiar experience of 'passage' in every interval and to assume that 'this content' it is that makes us speak of 'one and the same interval' (the pair of tones being of a certain relative frequency)<sup>3</sup>.

These three points call for some comment. (1) It may readily be

<sup>1</sup> "Das Erkennen sukzessiv gegebener musikalischer Intervalle in den äusseren Tonregionen," *Ztsch. f. Psychol.* 1913, LXIV. 161 ff.

<sup>2</sup> Cf. above, p. 31 f.

<sup>3</sup> *Op. cit.* 197.

admitted that fusion in the sense of the simultaneous representation of the successively presented tones had nothing to do with the judgments given by the observers. But it has not been shown that such relations of tones as account for their fusion when simultaneous, do not form the ultimate means of the standardisation of 'distances' that converts them into 'intervals'.<sup>1</sup> (2) It may also be admitted that intervals in this strict sense are not judged by reference to 'distances,' and yet it has not been shown that distances are not always involved and included in intervals. Intervals are distances that have somehow been standardised. (3) The time is past when qualitative differences can be established by a mere assertion, unless we are to use the term 'qualitative' in the loose sense of 'distinctive' or 'peculiar.' The bare assertion, therefore, carries no weight and may be dismissed. Moreover, on any theory, some explanation of the relation between 'distance' and 'interval' is called for, and this explanation will do much to settle what the psychological nature of the essential experience of interval is. I should not hesitate to assert that a proper classification of any 'peculiar' experience can only be given with the help of a systematic theory of the relations and connexions of the members of the large group of experiences to which it belongs.

I am not acquainted with any analysis, except my own, which suggests a ready explanation of the distance between tones, their interval, and the relation between these two things. Tonal distance, as I take it, is the parallel of visual or tactual distance. It is the crude, primitive 'mode,' the first and simplest of those that are founded on ordinal differences. Interval is, in general, a 'musical' term, a notion of the developed, discriminating, systematized, tonal consciousness<sup>2</sup>. Its nature is best defined by its simplest relation to distance; it is in the first place distance standardised. I do not mean to say that it is recognised or named by reference to distance, or that a judgment of interval is up to some point *eo ipso* a judgment based upon tonal distances; but it is a judgment which ultimately affects or concretes distances by standardising them. Frl. v. Maltzew's theory at the most merely points more correctly to the direction in which we shall find the means by which this standardisation is accomplished. It must be procured by the identification of something that is identical in intervals in spite of the differences of distance. She asserts—and I think it should now be

<sup>1</sup> Cf. above, p. 31 f.

<sup>2</sup> Cf. This *Journal*, 1911, iv. 180, "In talking of interval in the primitive sense we cannot mean consonant, dissonant or 'tonal' intervals."

clear that her view is merely assumption or assertion—that the basis of identification is the identity of the ‘step’ or ‘passage.’ But she does not show why the step from  $x$  to  $y$  is different from that from  $y$  to  $x$ . She does nothing to explain by reference to the familiar facts of other senses what the step or passage from one tone to another is. We learn much from her as to the connexions of similarity and familiarity amongst intervals and as to their relation to the memory. But that does not illuminate the problem as to their nature.

My analysis can only welcome, and be welcomed by, the results of Frl. v. Maltzew’s careful study. It would point to the identical relations of form constituted by successive intervals as the basis of the standardisation of distances, and therefore as the true constituents of intervals. Simultaneous intervals are also marked by peculiarities of form, which, though they differ from those of successive intervals, are derived from one and the same source. I have already pointed to various reasons which will make simultaneous interval a very different matter from successive interval. The greatest difference lies in the fact that in simultaneous interval two ‘systems’ of sound are summed to make a new system, with a definite form, containing two special maxima, various less prominent maxima due to difference tones, etc., and in the case of many dissonances various other oscillating features. These features appear in successive intervals at most only for an instant at the onset of the second tone, if the time interval between the two tones is less than a certain amount, and they must be much blurred and obscured. Successive interval must be marked, as Frl. v. Maltzew assumes, by the special features of a passage from one system of sound to another. But here each system is given separately. The form of the whole unity of the two systems of sound is therefore not that of a combined system, but of a succession. I do not see how on any theory this complex of form (successive) could be turned into the other combined (simultaneous) unit by means of representation, unless the other simultaneous complex had been identified as containing the pitches given in the successive form, and the two had been correlated and were reproducible through memory. We might as well expect the untrained eye to draw the sum of two sine curves without calculation. Confusion of thought about the relation between simultaneous and successive intervals has resulted from want of clearness regarding the voluminous nature of sounds and the nature of the analysis of sounds made by the ear. As I have shown above (page 36) by reference to observation and by deduction from my analysis, only pitches can be



analysed perfectly in simultaneous intervals, whereas intensities and volumes can only under special circumstances be separately gauged. But in successive intervals pitch, intensity, and volume all stand out clearly and separately in the successive 'systems' of sounds. The difficulty of holding, of recalling, and of singing certain of these intervals is the difficulty of passing from one sound system to another; it is clear that the presentation of the interval must largely determine the standpoint of the observer within the total combined system. The inversion of an interval means a great change of form unity. Identity of interval means identity of the form unity which the two successive systems form.

This unity may, of course, contain various secondary features which might serve as a basis for its recognition. Fr. v. Maltzew does not refer to any of them; they may, indeed, be wanting in any number of cases. She has shown the various means used by her observers to retain an interval and to make it clearer for recognition, such as transference to lower octaves, movements of the larynx, etc. All these aids presuppose the recognition of the peculiar form unity of successive interval. Only in the case of inference from recognition of absolute pitches is the form unity ignored. Direct and immediate estimation of interval<sup>1</sup> is the product of much previous memory work: correct classification presupposes processes of comparison of a more or less direct kind, which, as Fr. v. Maltzew has shown, still come into operation where intervals have to be judged in unfamiliar regions of sound. At the extremes of the tonal scale the form unities of intervals become very large and very small, and only by reference to the distortion of forms which for obscure physiological reasons then ensues, can we expect to explain the slight falsification of pitch which is found in the extremes of the tonal scale<sup>2</sup>. This normal false hearing does not necessarily imply a falsification of auditory distances, but only in the first place a disturbance of the usual processes for the standardisation of intervals and for the naming of tones which thereby becomes possible.

If both successive and simultaneous intervals involve attention to

<sup>1</sup> As also of pitch, of course, for the naming of pitches must be a result of the standardising of intervals. Thus is the primitive ordinal difference between tones converted into a musical system. It must now be clear how superfluous such an hypothesis as that of the 'counting cell' (*Zählzelle*) is. Musical evolution, moreover, does not require the assumption of any rapid development of the sense-organ of hearing, but only the construction of varied and thorough systems, based upon such standardisation, of greater and greater complexity.

<sup>2</sup> Cf. Maltzew, *op. cit.* 216 ff.

matters of tonal form, it becomes clear that tonality is the highest expression of the maintenance of continuity of form. Some basis for continuity of attention must be given, if complex sequences of sounds are to be apprehended rapidly. The more complex the nature of simple intervals is shown to be, the more necessary does this support to the attention appear. It may indeed be true that unity and proportion of form are made more noticeable by the increase of the complexity of the masses of sounds and the tonal forms they create; just as the unity and proportion of visual schemes are more effective in the larger visual works of art than in the simple parallelograms, triangles, and crosses of experimental analysis. But even so the attention of the observer must be guided in both of these arts and the more so perhaps in the successive structures of music. Tonality, or the introduction of a general scheme or system of intervals, needs no further justification in general than this. Only when we turn to the details of the schemes of tonality we actually find before us, do we need to enquire into the particular causes that have led to the formation of each particular scheme. The outlook that must guide these enquiries is clearly indicated in the psychological analysis and theory of hearing I have advocated.

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# ARE THE INTENSITY DIFFERENCES OF SENSATION QUANTITATIVE? III.

BY

HENRY J. WATT.

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# ARE THE INTENSITY DIFFERENCES OF SENSATION QUANTITATIVE?<sup>1</sup> III.

By HENRY J. WATT.

1. *Which differences of sensation do we call intensive?*
2. (a) *What psychological place does intensity occupy amongst the attributes of sensation?*  
(b) *In what relation does intensity stand to those modes of experience which bear a close psychological affinity to sensation and its attributes?*
3. *What is meant by the term 'quantitative'?*
4. *Is intensity a multitude or a-magnitude?*
5. *What other objects besides intensity are at least magnitudes?*
6. *Can intensity possibly be treated as a multitude?*
7. *The source of the confusion.*

THIS question may be specialised into a series of questions. The answers given to them will not only indicate the special points at which differences of opinion may legitimately arise, but will also show that certain differences are due to a confusion of ideas and may therefore be eliminated.

1. Which differences of sensation do we call intensive? It is agreed, I think, by all that the classification of certain differences as intensive cannot possibly be called in question. The cutaneous, muscular, gustatory, olfactory, and auditory sensations all possess the undoubtedly similar attributes of intensity. We may, of course, enquire whether intensity is native to all these groups of sensations and, if not, how they came to acquire it. But that it is there, is surely not disputed. Nor does the absence of any marked degree of variation of intensity, as for example in the articular sensations, really present a difficulty. The only important problem in this connexion is whether the particular case of visual brightness is to be classified as a form of intensity or as a form of quality or the like. But we can afford to neglect this problem

<sup>1</sup> A contribution to the Symposium presented at the Joint Meeting of the British Psychological Society, the Aristotelian Society, and the *Mind* Association in London, 7 June, 1913.

here and to confine our attention to the accepted cases of intensity. If visual brightness is to be considered intensive, the conclusions which are obtained for accepted forms of intensity will apply to it. But it is of interest to notice in passing that the proper classification of the attributes of sensation is not a perfectly simple task. There is room for serious divergence of views even at the present time<sup>1</sup>. Introspection is, of course, the only ground upon which a true classification can be founded. But it must be granted that the first, or in fact, any single, deliverance of introspection about the inherent nature and connexions of experiences is not necessarily irreproachable. We have to learn to think truly about simple experiences, just as about the objects of the physical world we live in.

2. (a) What psychological place does intensity occupy amongst the attributes of sensation? In discussing whether intensity is quantitative or not, reference is frequently made to extensity, as if the latter were undoubtedly quantitative. A certain amount of prejudice against a negative judgment regarding intensity is thus created. If this prejudice is misleading, it must be removed. I do not think that extensity can legitimately be considered to be a variable attribute. It is invariable. It is not really less present in the sensation from a 'spot' than in that from an area; there is not more of it in a square inch than in a square centimetre of colour. Nor is a low tone properly more voluminous than a high one. What there is more of in these cases is extent or volume, not extensity or voluminosity. We have indeed said for long enough that low tones are more voluminous than high ones. We had perhaps good reason to fear a confusion between the volume of a tone and the volume of the physical material, if we had used the same term for both. But nowadays this confusion can hardly occur in reference to the study of sensation. It is no longer from without, but within the field of psychology that the danger appears.

The variant commonly referred to under the name of extensity, voluminosity, and massiveness, then, is not an attribute of sensation. It is a derivative, a higher product, a *Gestalt*, like that of a line or a curve, and it is variable in the sense of being greater or less, like these. The attribute of extensity<sup>2</sup> is the common basis of extensiveness, the real ground of fusion and continuity of sensation in the midst of differences of local sign and its analogues, position and pitch, which I prefer to group under the generic name of order<sup>3</sup>. If the same

<sup>1</sup> Cf. my discussion of pitch and other cases in this *Journal*, iv. 843 ff.

<sup>2</sup> I hope to deal with this attribute more fully at another time.

<sup>3</sup> Cf. *op. cit.*



distinction is applied to the attribute of temporal extensity or duration, we obtain the following grouping of the attributes. Quality and intensity stand apart from the others, which fall into two pairs. Each pair comprises an extensive and an ordinal member and the two pairs may be named temporal and systemic. But, however tempting it may be for the purpose of systematic appearances, it is impossible to treat either quality or intensity as extensive or as ordinal in character. They are both ordinal in the sense of being self-disposing, but this peculiarity of them cannot be identified with ordinality; for upon differences in quality or in intensity none of those *Gestalten* or modes of sensory experience are founded which grow upon ordinal contents, viz. distance or interval, motion and others; and besides, quality and intensity are both more than merely self-disposing.

(b) In what relation does intensity stand to those modes of experience which bear a close psychological affinity to sensation and its attributes? These modes of experience have been forcing themselves with ever increasing insistence upon the notice of psychologists. There can be no doubt about their enormous variety and importance. Since the leading paper by Chr. v. Ehrenfels in 1890, by whom they were called *Gestaltqualitäten*, a large number of studies have been made of them<sup>1</sup>. Such modes of experience are said to be founded upon contents, which may either be other modes or in many cases elementary sensations. I believe that there is always a certain amount of resemblance between the founded mode and its founding contents or some aspect or attribute of the latter, as well as an objective psychical dependence of the mode on its founding contents. These relations seem to me to form good ground upon which a body of pure psychological theory concerned with the interconnexions and development of experience may be built up<sup>2</sup>. Many modes are variable and self-disposing, as being greater or less than one another in respect of their own peculiar phenomenal content. Distance and interval of time and motion are amongst the simplest of them, but there are many others<sup>3</sup>. The full and adequate study of these modes, their variety, relations, and theoretical explanation, is one of the newest forms of the psychological task, and will undoubtedly show itself to be one of its bulkiest parts.

In this connexion I see reason to differ from certain views indicated

<sup>1</sup> The first volume of a most valuable and important work by Karl Bühler on *Die Gestaltwahrnehmungen* has just been published (1913).

<sup>2</sup> Cf. my paper on the "Psychology of Visual Motion," in this *Journal*, vi.

<sup>3</sup> This *Journal*, iv. 157 ff. For other modes cf. Bühler, *op. cit.*

by Dr Myers in 1. § 2. The modes which stand next to elementary sensation seem to be, first distance and time-interval, and then, as a combination of these two, motion. Spatiality, if it is merely simple distance, seems to me to be in the matter of psychological origin independent of motion; if it is complex, such as is the spatiality of binocular vision, it does not seem to me to involve motion as a necessary psychological antecedent at all. Nor do I see any evidence for the existence of a psychological antecedent to intensity, simpler than intensity, from which intensity might arise by the integration of two or more of its varieties, as distance may be said to be integrated out of differences in the attribute of order. Any other speculations regarding the origin of intensity seem to me to be either inventions or to rest upon mistaken correlations.

3. What is to be meant by the term 'quantitative'? It seems to be agreed that there are two possible meanings. A quantitative object is either,

(a) A collective object, whether real or ideal—a number of material particles, persons, states of mind, events, or a number of ideal numbers, lengths, forces, universals. Let us call this kind of object a multitude.

Or (b) A self-disposing object, or an object say  $a_e$ , which in virtue of its own phenomenality disposes itself amongst other objects of the same group  $a_b, a_c, a_d$ , etc., in a definite manner, so that it falls between  $a_r$  and  $a_t$ , and not between  $a_d$  and  $a_f$ , and which in these relations appears to be greater than  $a_r$  and less than  $a_t$ . This kind of object is known as a magnitude.

4. Is intensity a multitude or a magnitude? [With regard to the expression 'intensity differences' in the title of this discussion, I take it to mean, in the first place, intensities, and only in the second place, if at all, differences of intensities, such as those between  $Ia$  and  $Ib$ ,  $Ib$  and  $Ic$ .] On two points there seems to be agreement: (a) intensity is at least a magnitude; and (b) we cannot yet validly treat it as a multitude. We can, therefore, proceed to discuss the possibilities that are logically unaffected by these decisions. But before doing so it is well to turn aside for a moment and ask another question.

5. What other objects besides intensity are at least magnitudes? It is agreed, I think, that felt distance and motion and other such modes of experience or *Gestalten* are also at least magnitudes. We may, therefore, infer that the world of experience is rich in objects of this kind. Probably all forms of experience are, in some sense or to some degree, self-disposing objects. But a number of them cannot be considered to

be magnitudes, for example the above mentioned attributes of temporal and systemic order, percepts, recognition, concepts, thoughts and the like. The most obvious groups of experiential magnitudes are the modes and figures (*Gestalten*) of space and time, their combination in motions, and the various classes included under the term 'relations.' Magnitudes seem to occur by preference on what is obviously a duple or multiple foundation, such as we find in distance, succession, and change, or on what for various reasons may legitimately be held to be a duple or multiple foundation, as in minimal distances, motions, changes, etc. Feeling is one of the few cases in which a duple foundation seems to elude our grasp, but even here there is some sort of positive evidence<sup>1</sup>. But there is at least no reason to doubt that differences of intensities are magnitudes and that we find it comparatively easy to arrange them and to observe and to indicate their apparent equality. In so far as we consistently maintain their phenomenal equality, we have as much reason to believe in the validity of our introspective judgments, as we have to believe in them in other regions of introspective work. But if *a*, *b*, and *c* are not multitudes, but experiential magnitudes, we cannot suppose that judgments regarding the equality of the differences between *a* and *b* and between *b* and *c* justify the statement that the difference between *a* and *c* is twice that between either of the former pairs. For the judgment regarding *a* and *c* has no bearing on the other two judgments, and *vice versa*. All just noticeable differences are equal in being just noticeable, but that does not make them equal increments. Nor can equal differences be considered to be equal parts of another difference, *i.e.* equal increments within the latter difference. Is there any sense in calling the tone interval *g—f'* twice as great as that between *g* and *c'*, because the intervals *g—c'* and *c'—f'* are equal in being fourths? Besides, a distance is not the difference between two points, but these and the stretch between them in a unity.

It would carry me too far from the object of this discussion, were I to enquire whether any non-mental, for example, material or ideal objects, are at least or solely magnitudes. Nor do I think it would throw any light upon the object of discussion.

6. Can intensity possibly be treated as a multitude? The conclusion I wish to plead for in this discussion is that it cannot, so long as the identity of the object under discussion, namely intensity, is maintained. I would suggest that an object cannot at one and the

<sup>1</sup> Cf. my discussion of it in this *Journal*, iv. 184 ff.



same time be directly immeasurable and indirectly measurable, as Meinong<sup>1</sup> declares and as Professor Dawes Hicks<sup>2</sup> agrees. Such a proposition can have an appearance of plausibility only by the substitution of a new measurable object for the one that is directly immeasurable. This substitution may be occasioned by the close connexion of the two objects in the world of reality, but it is none the less a substitution. To speak of a surrogative form of measurement is both misleading and wrong. What the medical thermometer measures is not the patient's sensations of warmth or cold or how warm or cold he feels. In this particular instance the departure from any sort of regular correlation between magnitude of felt warmth and degree of temperature is notorious. What the physician is usually concerned to know is the temperature of his patient's body. And that is as little a surrogative measurement of his patient's feelings as the sight or taste of the physic he offers is a surrogative cure for his patient's felt discomfort, however much or little the material physic may be suited to restore the patient's body to its normal condition. No one sets out to measure the sensed distances evoked by a thermometer scale, but only the lines or lengths of that scale. The latter are measurable, as are any multiple objects, in so far as they produce regular changes upon lines or lengths. In all cases it is only that aspect of the motion of matter which by an obvious convenience has come to be called temperature that is measurable. And similarly in other such examples.

I would also submit that in every case in which the treatment of single states of mind as multitudes is in any way made to be plausible, we find a substitution of objects of the kind mentioned. So for example in Fechner's formula, which is perfectly valid in so far as  $S$  in the expression  $S = K \text{ Log } I$  means 'the numerical value of  $S$ ,' if it exists. But unfortunately this value has no real object; the object and the value are purely imaginary. The fault here does not lie in the application of mathematical symbols and processes to the data of sense; for these are most certainly applicable to the data of sense whenever we have an opportunity of dealing with multitudes of these data, *e.g.* in the statistical manipulation of records of the frequency of visual and other images, in the study of memory and so on. The error committed by Fechner consists in applying mathematical symbols and processes to the data of sense without any proper psychological or objective justification.

<sup>1</sup> *Ztschr. f. Psychol.*, 1896, xi. 239.

<sup>2</sup> Cf. pp. 168 ff.

There is no theoretical difficulty in discovering truths that are non-truths. The difficulty is always a 'practical' one. The truths 'wanted' are simply not there to be had.

The substitution of objects I speak of may also be illustrated from Dr Myers's main thesis that the physiological correlate of intensity differences is a sub-group of extensive changes. That may very well be, but the thesis, as it stands, cannot be considered to afford any interpretation or elucidation of intensity or its differences. If it is a valid hypothesis, it certainly establishes a fact, it discovers a reality, a new kind of extensive distribution of physiological processes; and it sets this reality into relation with intensity. But that is all. We are not thereby brought any nearer to a treatment of intensity as a multitude. We merely know now a relation in which intensity stands that we did not know before. It does not affect the case in the least that the object with which intensity has been shown to stand in relation is itself a multitude. Physiology can be said to throw light upon psychological matters only in so far as a sufficient number of these relations between experiences and physiological processes are discovered to warrant the inductive assumption that certain known physiological units stand in certain relations to known psychical units or that certain as yet unknown psychical units exist and are related to these known physiological units in certain ways. I do not by any means deny the possibility of this inductive procedure. But I very much doubt whether the reverse does not constitute the method of greater illuminative power.

In short, no single state of mind can be treated as a multitude, not even the idea of 100 itself. Only the object of the idea of 100 can be so treated. But I do not mean hereby to imply that every object can be treated as a multitude. We must, of course, discover and determine whether any given object can be so treated or not. If we succeed, the object is a multitude; if we do not succeed, it may often still be a multitude. We cannot tell *a priori* where we are to look for objects that are multitudes and where not. Otherwise psychologists have made a sorry waste of their time and energy. It is quite possible that someone may yet prove by new methods that behind intensity there lie psychical objects now unknown to us which are to be considered as multitudes and are responsible for the phenomenon of intensity (cf. Myers, I. § 2). But not even such a proof would enable us to look upon intensities as themselves multitudes. Such a magnitude as intensity, like the so much discussed and practically useful distance, must remain a magnitude for ever and ever.

This may be enforced by another illustration. It is possible to maintain that felt distance is *realiter* psychologically founded upon repeated (*i.e.* a multitude of) sensational elements qualified by extensity and order and that thus differences of multitudes are the real basis of the differences of magnitude found in distances. But not even that would make distance in any sense a multitude. Only its real psychological basis would be a multitude<sup>1</sup>.

If we had such as this imaginary knowledge of the real psychical basis of intensity, we might formulate the laws of mind and predict the psychical future better than we do now. But future mental states can be predicted by the knowledge of the physical world we already possess. We can, for example, arrange the illumination of a room so as to produce various mental effects. Yet that fact does not imply that we can measure intensity or its differences. Nor would the discovery I imagined.

If, finally, it be suggested that intensity can be treated as a multitude or measured by convention, I would submit that such 'measurement' is only a means of *naming* what stands in a real relation to something else that can properly be measured, as star brilliancies to the varying intensity of physical light.

7. The source of the confusion in these matters is an epistemological one—either a confusion of objects or a confusion of the immediate basis of knowledge in sensory experience with the objects of knowledge. In the latter case distance as sensed, for example, may be confused with length, felt motion or its velocity with motion through real space or the velocity of real motion. But it is surely absurd to suppose that any sort of reality—called velocity—exists that is a unitary magnitude in the sense in which colours and tones and felt velocity are such, and that nevertheless is measurable in numbers. Such a unitary reality is a myth, the hypostatization of a complex set of correlated relations in which a real or ideal object stands. Whether these relations are themselves real or ideal, actual or imaginary, makes, of course, no difference to the case.

If I rejoice in the possession of a new book, neither the possession nor the book thereby become feelings or emotions. If I know yonder tree is budding, neither the tree nor the budding thereby become either sensations, perceptions, or knowledge. They are only the objects of my knowledge and as such come into relation to my knowledge. So if I can

<sup>1</sup> Cf. the analogous theory given by E. R. Jaensch of the psychical representation of empty space, *Ztsch. f. Psychol.*, Erg.-bd. vi. 244ff.



measure lengths, why should I worry about not measuring distances as felt (*Gestalten*), when I have already ascertained that I cannot measure them? If lengths are in fact measurable, the equality or differences of distances may be the sensory basis on which the cognitive processes of conception and knowledge involved in the act of measurement build. But that is no reason why I should require or expect to be able to measure distances. If unitary distances are not to be converted into multitudes, we must just enquire how our cognitive processes can nevertheless make measurement of lengths possible. It is futile to think distances ought somehow to be measurable or to construe them so as to imagine them measurable. A real object has certain definite properties and it stands in certain definite relations to other objects; all one can do is to find out these things by knowing. Knowing powers will never by themselves alone change the properties of objects or set them into new relations, unless these be relations to my knowing or unless I somehow act upon the objects so as to change their real relations.

It seems necessary to make these remarks as there is a consensus of opinion that we actually do not succeed in measuring mental magnitudes such as intensity; and yet attempts are made to give the impression that after all our intellect is not so ineffective and useless as it is (most perversely) considered to be and that we really do measure these magnitudes; only we do not do the measuring in these cases directly or straightforwardly but indirectly or by substitution, or to put it bluntly by make-believe.



# THE PSYCHOLOGY OF VISUAL MOTION.

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HENRY J. WATT.

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# THE PSYCHOLOGY OF VISUAL MOTION.

BY HENRY J. WATT.

- I. *Criticism of Wohlgemuth's physiological theory.*
- II. *The introspective nature and affinities of the after-effect of seen movement.*
- III. *The correlation between the introspective features of the after-effect and those of the previous objective movement.*
- IV. *Wertheimer's criticism of certain psychological theories.*
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Two elaborate studies of visual motion have recently been published by A. Wohlgemuth<sup>1</sup> and by Max Wertheimer<sup>2</sup>. Both of these important papers add much to our knowledge of the facts, and excel in clearness and precision of work. They are also alike in rejecting all the psychological theories that have been advanced in their several fields of research and in formulating a physiological theory in explanation of the facts. Neither writer, however, makes any contribution to psychological theory. The possibility of such a thing is hardly even suggested; it is presumably annulled by the mere offer of a physiological theory. But the matter is not debated.

This situation seems to me so anomalous as to be worthy of special notice, the more so as the facts of the case hardly warrant the attitude adopted by these writers. In this paper I propose to deal briefly with the theory and outlook of these two works, which may be considered typical of a certain trend of opinion prevalent at the present time. In view of their general importance, however, and for the sake of brevity, I shall assume for the most part that the reader is already familiar with them and need only be reminded of their contents as each point arises.

<sup>1</sup> "On the After-effect of Seen Movement," this *Journal*, Monograph Supplement, No. 1.

<sup>2</sup> "Experimentelle Studien über das Sehen von Bewegungen" (Habilitationsschrift, Leipzig, 1912), *Ztschr. f. Psychol.* Lxi. 161 ff.



## I. CRITICISM OF WOHLGEMUTH'S PHYSIOLOGICAL THEORY.

The after-effect of seen movement, which is the object of Wohlgemuth's investigation, is familiar in various natural situations. If fixedly we gaze at a streaming waterfall or look down upon a rushing river for half a minute or so and then turn to look at the ground, the latter will seem to be streaming in a peculiar manner in the direction opposite to that in which the water flowed (relatively to our field of vision). For experimental purposes a simple form of this process is devised. A sheet of paper bearing alternately black and white lines of some little breadth is fixed upon a drum, which is rotated so that the lines move across the field of vision more or less slowly in a direction perpendicular to their length. Under suitable circumstances the lines will appear to move backwards when the motion of the drum is stopped. As the eye has been fixed and steady all the time, this peculiar after-motion cannot be due to any motion of the eye after the stopping of the drum, but must be taken as the after-effect of the preceding motion. Some theorists have therefore supposed it appeared because we were deluded by the previous objective movement into being accustomed to motion and therefore into expecting motion for a longer time than it was really there and thus into seeing what we expected. But this can easily be disproved by the application of incognitive methods, which prevent us from knowing from time to time what really happens. If the same after-effect follows whether the observer knows what is really happening or not, it cannot be the result of an illusion of judgment.

If the mind does not work at all to produce this after-effect, then apparently the only task for theory is to extend the accepted notions regarding the general physiology of neural processes so as to cover the facts; or to imagine a neural mechanism which will shew why motion is sometimes perceived where nothing really moves and why it then runs in a certain direction, opposite to that of the preceding movement. For his theory Wohlgemuth assumes that retinal elements  $a_1$  and  $a_2$ ,  $b_1$  and  $b_2$ , are each connected with a "subcortical centre of movement," consisting of summation cells  $A_1$  and  $A_2$ ,  $B_1$  and  $B_2$ , and also in pairs with a *Schaltzelle*  $S_1$  for the  $a$ 's and  $S_2$  for the  $b$ 's.  $A_1$ ,  $A_2$ , and  $S_1$  are also connected with one another, as are  $B_1$ ,  $B_2$ , and  $S_2$ . Impulses are sent by  $A_1$  and  $A_2$  to the cortex, but this system of centres of movement is independent of other centres, *e.g.* those for brightness, colour, local sign, etc.

(a) Wohlgemuth assumes (pp. 99 ff.) that in the hypothetical centre of movement, owing to the part played by the *Schaltzelle* *S1*, a state of facilitation lasts in *A1*, so long as the objective movement stimulates the eye, but that as soon as this movement is stopped, the state of facilitation in *A1* is replaced by a state of fatigue in *A1*. By this means, during the objective movement, *A1* is more excited than *A2*, while during the after-stage *A2* is more excited than *A1*. The psychical counterparts of these relations of intensity are, for the former, movement having the direction *A1*—*A2*, for the latter, movement in the opposite direction.

Now *this assumption posits the unfailing occurrence of so special a case that it seems to me to vitiate the whole theory.* We should rather expect many possible relations between facilitation and fatigue: facilitation frequently still increasing with psychical counterpart of similarly directed movement and after-effect, occasional balance with no visible movement or after-effect, frequent fatigue after longer stimulation with a reversal of both seen movement and after-effect, and thereafter periodic return to a state of balance. But, as we read on page 85, "no after-image of the sectors moving in the same direction as the objective movement could at any time be detected."

(b) *The theory offered virtually begs the question.* For, in order to suppose that the physiological basis of the experience of pure motion exemplified by the after-effect is a difference of excitation amongst the cells *A1*, *A2*, etc., and that the physiological basis of the direction of the felt motion is the spatial distribution of this difference of excitation amongst the cells *A1*, *A2*, etc., it must assume that the cells *A1*, *A2*, etc., already function as the physiological basis of different localisations, and that real directions within the complex of cells *A1*, *A2*, etc. (with or without actual physiological connexions between these cells) form the basis of felt directions; or it must assume that the cells *A1*, *A2*, etc., individually and as a complex, are connected and correlated with those other centres that are the physiological basis of localisations and directions. In either case the theory takes the physiological basis of localisation and direction for granted and only offers a theory of motion, treating it as a sort of intensive state, which refers and is attached to these localisations and directions, and endows the "sukzessive Aufspringen eines gleichartigen Eindrucks an verschiedenen Orten"—which we might perceive merely as such, were we beings devoid of the peculiar experience of motion—with this

unitary quality of continuity, namely "ein Hindurchgehen durch die zwischenliegenden Räume<sup>1</sup>."

But can motion really be treated in this way? Is it not rather the case that motion has a direction of its own, which may coincide with, or be opposed to, some other direction of which we are conscious apart from any motion? Is not also the velocity of a motion a characteristic of its own? Are not the motion, the direction, and the velocity, of motion—whether it correspond to a real motion or be pure motion in the sense of the after-effect—the essential aspects of this experience, its vividness being necessary in some degree, but as such relatively unimportant? The vividness of the experience may be to some extent interchangeable with its velocity, in so far as an increase in velocity is accompanied by an increase in vividness; but surely it would be contrary to experience to allow this vividness to usurp the place of the velocity of the motion itself, not to speak of its direction. And if motion presents a continuity that is not given in, or derivable from, the data of our space and time *Anschauungen*, should we not expect to find an explanation of this continuity included in the physiological theory of motion? But it is evident that this continuity is taken for granted in the theory as stated.

It seems then that the theory in question offers an explanation really only of the vividness of the experience; and if against this must be written the arbitrary assumption which I have stated under (a), the balance leaves nothing to the credit of the theory. We must discover first of all what is the neural basis of pure motion, its direction, and its velocity; it will hardly be very difficult thereafter to find a basis for its vividness.

On page 19 Wohlgemuth says that Borschke and Heschel admit that, as seen, the movement of two sets of straight rods at right angles to one another "can only be regarded as squares, moving in an oblique direction." This movement can, of course, be described as one pleases; but if it is felt as the movement of squares in an oblique direction, that must be due to psychical, or shall we say, central, reasons; for it is essentially the *Gestalt* of the square which determines the apparent movement. If a point on one of the rods were marked out by colour or shape, we should at once in so far be free from this apparent oblique motion of the squares<sup>2</sup>. In connexion with this the

<sup>1</sup> Cf. Ebbinghaus, as quoted by Wohlgemuth, *op. cit.*, p. 108.

<sup>2</sup> Cf. Pleikart Stumpf, "Ueber die Abhängigkeit der visuellen Bewegungsempfindung und ihres negativen Nachbildes von den Reizvorgängen auf der Netzhaut." *Ztschr. f.*



forced explanation given of the results of experiment 28 on page 107 should be consulted.

(c) *The theory constitutes, as it stands, a lapse from the presumable parallelism of mind and body*; it fails to shew that the relations of mind and body, whatever they may be, follow any general scheme or plan; in fact, it suggests that they vary arbitrarily from one experience to another. For all would agree, I think, that the neural basis of the arrangement of the simplest sensory experiences in respect of their adherent localisations is, proximately or ultimately, the arrangement of neural units of some kind. Of course, we should not expect to be aware of the experiences correlated with these neural units, nor of their localisations, apart from some degree of excitation in these neural units. But neither should we expect to find that the essential aspect of their stimulation, with which alone experience is correlated, is the difference of excitation in them. For even if difference of degree of excitation were a necessary feature of the neural basis of the experience of motion, and of its direction and velocity, these experiences must first and foremost be correlated with the arrangements and interconnexions of the neural units and only secondarily with their difference of excitation. Difference of excitation would, then, be only a means of bringing different localisations with different clearness and insistency to the mind.

Thus we might revert to the simple theory of common sense and expect motion to be based upon the successive stimulation of neural units correlated with different positions. And it is to be noted that we have as yet no evidence that bears against this view or shews that the effect of motion is producible from simultaneously stimulated neural units, be they stimulated equally or differently. The facts of the after-effect of seen movement do not, of course, afford this evidence. They offer no other evidence than do the ordinary facts of motion. It is only in the eyes of such a theory as Wohlgenuth's that the stimulation of the neural units subserving motion is simultaneous and different. [When the stimulation of the elements of a neural complex in different degrees is said to be simultaneous, that means, of course, for Wohlgenuth as for others, simultaneous and continuous over a short stretch of time.] But he extends this explanation not only to the

*Psychol.* LIX. 324: "Im Vorbild stimmte die gesehene Bewegung nur nicht dann mit der berechneten Richtung überein, wenn irgendwelche Anhaltspunkte andere Auffassungen begünstigten," etc. Compare the effect of using broken lines and spirals, where the seen movement always corresponds to the objective movement.

after-effect of motion, but to ordinary visual motion. His hypothesis thus stands in sharp contradiction to the facts upon which it ultimately rests, that is, both to the facts of experience and to the facts known regarding the elements of the peripheral stimulation and their relative qualities, intensities, positions, and times. The positions of these elements differ in different times, so that in the several neural units stimulated by them, at least in those proximate to the stimulation, there must necessarily be successive differences, be they differences of intensity or of quality or of both together. Is it not, then, most reasonable to suppose that whenever motion is given, these successive differences occur throughout all the elements of its neural basis, be they proximate or remote?

Therefore it seems that Wohlgemuth's physiological theory of motion fails to shew that the relations of mind and body follow any general scheme or plan; or if it does so implicitly, it places a false emphasis on the part played by the intensive differences of neural processes in the correlation of mind and body.

## II. THE INTROSPECTIVE NATURE AND AFFINITIES OF THE AFTER-EFFECT OF SEEN MOVEMENT.

The first task of psychology seems to me to be a thorough study of all distinguishable varieties of experience and their arrangement on the basis of their resemblance to one another, whether the resemblance be that of appearance (*e.g.* of attributes) or of functional properties and variations. We must form a periodic table of experiences, as it were, and we must take that table as the basis and object of explanation of every theory which is to be called psychological.

From Wohlgemuth's valuable historical and experimental researches it appears that the after-effect of seen movement has the following characteristics or properties<sup>1</sup>:—

- C* 1            The after-effect is an apparent movement, in a direction opposite to that of the previous objective movement.
- E* 16           Its velocity is comparable with that of an objective movement.
- E* 15           Its velocity acts as a velocity. It adds itself to an objective movement.

<sup>1</sup> *C* refers to conclusions by agreement between Wohlgemuth and his predecessors, *E* refers to Wohlgemuth's own experiments (the numbers are those of his text), cf. pp. 110 ff.

- E 33* Certain observers mistake it for a real objective movement and are unwilling to believe the contrary (p. 87).  
*E 10—13* It varies in vividness (cf. pp. 46 ff.).  
*C 3* It is definitely localised.  
*C 4* It has a definite position in time.

These are its positive features. Negatively it appears that :—

- E 32* It is not like "a shadow passing across the stationary surface."  
*E 33* As compared with an objective movement it has a hollow ghost-like appearance. Or it may have all degrees from reality to evanescence and ghostliness. As Wohlgemuth observes it, it is an experience *sui generis*. For him it never approaches the appearance of real objective movement. It lacks the solidity and reality that is given by change of position in space (cf. pp. 87 f.).

We must, therefore, conclude that, no matter how unusual the isolation of pure motion in the after-effect may be, nor how "unreal" it looks, it does greatly resemble its prototype of objective movement, fusing with the latter both phenomenally and functionally.

### III. THE CORRELATION BETWEEN THE INTROSPECTIVE FEATURES OF THE AFTER-EFFECT AND THOSE OF THE PREVIOUS OBJECTIVE MOVEMENT.

The next question is whether the introspective kinship thus established is confirmed or contradicted by the evidence regarding the correlation between the introspective features of the after-effect and those of the previous objective movement upon which it is dependent. With which feature or features of simple sensation is the after-effect objectively connected? We may pass in review the chief attributes of sensation: (a) quality, (b) intensity, (c) order (local sign), (d) position in time, (e) extensity, and (f) duration. Of these, however, only the first four really come into question.

(a) *Quality*. The relevant facts are these :—

- E 17—18* "The after-effect is independent of the quality of the light." The latter may be varied without variation of the former. Cf. *E 19* below.  
*E 29* "Fatigue produced by alternating movements of opposite sign is independent of the colour of the light producing it, *i.e.* the fatigue is maintained in light of different colour."



*E1*, *C11*, and *E2—4* may also be cited, which shew the manner in which the clearness and vigour of the contents of the visual field reinforce the vividness of the after-effect. The after-effect is also noticeable in the dim field of subjective vision (eyes closed).

The conclusion, then, must be that the after-effect cannot well be produced apart from quality of some kind, but it is independent of the variation of the quality, as such, of the light. It is presumably produced by a factor which accompanies quality and which becomes, to some extent, more insistent as quality becomes more insistent. The explanation which Wohlgemuth offers of *E17—18* that "each new colour is a new stimulus" (p. 106) hardly seems consistent with *E29*. Wohlgemuth's theory may explain the latter, but it can hardly explain the former. Wohlgemuth himself seems to feel this difficulty (cf. pp. 107 and 109).

(b) *Intensity.*

- E19* "In the case of different colours difference of brightness is not essential for the production of the after-effect."
- E14* "If a moving series of alternating dark and light stripes excite the retina, a slightly better after-effect seems to be obtained if the stripes be of equal width; but if the alternate dark and light stripes be not of equal width it seems not to matter which stripes are increased and which decreased in width."
- E2—4* "The after-effect is more marked in a brightly illuminated objective field...than in a darker field."
- E5—6* "If during the passage of images over the retina, a stimulus of a given intensity alternates with one of less intensity, the after-effect of movement produced is more vivid than if such stimulus alternates with a (more or less complete) cessation of stimulus."
- C1* "The uniform passage of light stimuli over the retina in any given direction...produces the after-effect."

The decisive case is *E19*, which shews that a variation of intensity is not an essential condition. *E14* is only compatible with Wohlgemuth's theory if the special assumption discussed under *Ia* is admitted. The other results, along with those referring to the difference between the light- and the dark-adapted eyes, are concomitant variations, which may depend not only upon the variations of intensity, but upon that of one of the other attributes. The

explanation of *E* 5—6 which Wohlgemuth gives (p. 104) seems strained: "When a black stripe succeeds a white one the synapses, which had been fatigued, immediately regain their former state." But the main theory supposed these synapses to be in a state of facilitation. If they are fatigued, *A* 1 should be more fatigued than *A* 2, having been excited more strongly longer, and the movement should have turned apparently to the direction opposite to that which it shewed at first.

The conclusion then must be that the after-effect cannot well be produced apart from intensity of some kind, but that it is independent of the intensity, as such, of the light. It is presumably produced by a factor which accompanies intensity and becomes to some extent more insistent as intensity becomes greater. If the after-effect is to be got, moreover, either the quality or the intensity must be varied. Both of these may, but need not, be varied at once. The after-effect, therefore, cannot well be dependent upon either of these attributes, but it may be dependent upon a factor which changes with differences in either or both of these<sup>1</sup>.

(c) *Unocular order* (local sign).

*C* 1           Quoted above.

*C* 2           "This after-effect is more marked if the eyes...remain fixed on a stationary point."

*C* 8           "The after-effect is producible by any rate of the stimulating movement."

<sup>1</sup> It is necessary to refer at this point to the preliminary notice of experimental results issued by Pleikart Stumpf, in which he says: "Es zeigte sich nämlich zunächst die auffallende Tatsache, dass bei sukzessiver Helligkeitsänderung einer Farbe des einen Farbenpaares sich eine Stelle finden lässt, bei der der Bewegungseindruck in den meisten Fällen vollkommen verschwindet, oder in einigen besonderen Fällen doch ein Minimum an Deutlichkeit erreicht. Zu jeder Farbe lässt sich auf diese Weise ein bestimmtes Grau finden, das mit ihr, so müssen wir wohl annehmen, einen unwirksamen Erregungsübergang bildet, so dass kein Bewegungsempfindungsprozess mehr zustande kommen kann" (*op. cit.*, 323 f.). The grey is that which gives the lowest fusional frequency with the colour concerned. If Stumpf's observations are correct, their inconsistency with those of Wohlgemuth may be the result of the difference of method adopted. Stumpf's method is essentially stroboscopic and his bands of colour are very narrow—two millimetres. No account is given, however, of the means of obtaining the necessary variation of brightness in the grey bands, which to give the result stated must have been most laborious. Until full details are given, Stumpf's result must be held in suspense. In view (1) of the restriction of a "minimum in some cases" which he indicates, (2) of the absence of any reference by Stumpf to differences of velocity, and (3) of the cumulative effect of certain differences in Wohlgemuth's results, Stumpf's case must be supposed to be an exceptional one. At all events, the theoretical procedure upon which alone I wish to insist here, must be applied to all relevant and stable experimental results.

- C* 10 "Pseudo-movements, *e.g.* stroboscopic movements, produce an after-effect exactly as an actual movement does."
- C* 7—9 "The after-effect increases in one or several ways, within limits, with the number of stimuli simultaneously affecting a given area o. the retina, and or with the frequency with which the stimuli pass given retinal elements."
- E* 10—13 "The after-effect at first increases very rapidly with the objective velocity, but soon reaches a maximum and then gradually diminishes with further increase of speed."
- E* 14 Quoted above.
- E* 21, 1—4 In the periphery of the field of vision the after-effect is at first more vigorous, but diminishes and disappears very rapidly.
- E* 21, 5 "Any after-effect in a not-stimulated area is of opposite direction to that of the stimulated area." [Not weaker or less rapid.]
- E* 5—6, 1 Here Wohlgenuth says that "distinctness of contours is not the essential factor in the production of the after-effect." But it is evident from page 37 that "distinctness of contours" is only an alternative reading for "difference of brightness."
- E* 28 "After fatigue has been produced by a long series of movements alternating in sign (so that the after-effect is greatly reduced), the after-effect of movements at right angles to the direction of the previous ones is only very slightly affected, if at all."
- E* 26 "When several objective movements of different directions stimulate the same retinal area simultaneously or successively, an after-effect is produced which is the resultant of the after-effects of the various movements."

*C* 1 obviously admits the influence of order and *C* 2 provides a better basis for its regular introduction. Contour is the chief firm of accentuation of visual position, so that the greater the number and frequency of the moving contours the greater the variation of orders (*E* 7—9). The impression of motion comes into full effectiveness more or less suddenly after a certain rate of motion has been obtained, but it becomes less clear with the higher velocities (*E* 8, *E* 10—13). *E* 14 calls for the operation of a factor which is independent of the division of the period between the light and dark portions. It is, on the other



hand, a well-known fact that orders and distances are clearer when they are regular and symmetrical. *E* 5—6, 2, which shews that a grey stripe is more effective in alternation with a white one than a black one is, may be supposed to involve a greater clearness of orders. For when black and white are juxtaposed, they must intensify each other by contrast, and so make irregularities of brightness of their surfaces less noticeable than they would be if the black were replaced by grey. That is to say, grey favours the distinction of positions, or, in other words, it allows of the existence of many orders, besides that given in the contours. With *E* 26 we may compare what was said above about the apparent movement of squares in an oblique direction, when two sets of parallel rods move at right angles to one another. If the after-effect is correlated with the neural basis of orders, directions, and motions, there is no reason why fatigue for one direction should affect the receptivity towards another direction at right angles to the first (*E* 28). Wohlgemuth's explanation of this result, on the contrary, must be said to be highly strained (*vide* p. 107). As regards *E* 21, 1—4, it is a commonly accepted fact that motion is more insistent in the periphery of the field of vision, but that positions there are not so highly differentiated as in the centre of the field. We might, then, expect a more insistent after-effect of briefer duration, rapidly disappearing. It is difficult to see what relation *C* 10, especially as described by Wertheimer, has to the varying intensity of pairs of movement centres. But their relation to differences of order and of time is obvious.

The conclusion must, therefore, be that the after-effect is correlated with, and directly or indirectly dependent upon, the order-differences of sensation given by the objective movement which excites the after-effect. There is no fact which suggests that the after-effect is independent of this attribute of simple visual sensation. *E* 1 and *E* 2—4 only imply that the presence of clear qualities and high intensities involves clearer sensational orders than does a darker or obscurer field.

Of *C* 9 Wohlgemuth says: "This result is probably merely a question of fusion of two retinal fields like results Nos. [*C*] 6 and 7" (p. 103). These binocular cases do surely belong to quite a different class of integrative processes to be studied separately from unocular cases.

(*d*) The only other attribute which could come into question at all is that of *position in time* which represents rate of succession of stimuli.

It is undoubtedly involved in the production of motion and its after-effect, both in Wohlgemuth's theory and in any other. It is definitely involved in *C* 1, *C* 8, *C* 10, *E* 7—9, *E* 10—13, and *E* 14. Neither motion nor its after-effect is to be correlated with simultaneous sensations.

The following results do not apparently favour or disfavour any particular theory of the psychological or neural basis of motion and its after-effect: *C* 5, *E* 22, *E* 25, *E* 27, *E* 30.

The preceding investigation thus bears out the suggestions given by the psychical affinities of the after-effect of motion. This not only resembles motion, but it is related by direct psychical correlation with the experience of motion evoked by preceding objective movement, and with the conditions which favour or indicate a greater clearness of the orders of the sensations aroused by the objective stimulus. I offer no physiological theory alternative to that of Wohlgemuth. Nor do I mean to suggest that the after-effect of seen motion is linked to the preceding objective movement by any bonds of psychical causation. But I would maintain that the introspective nature of the after-effect is such that it resembles motion and order, while the correlations which experiment has established between the objective motion and the after-effect are such as to lead one to believe that the physiological basis of the after-effect is identical with that of motion and that both are connected with, and dependent upon, the physiological basis, not of intensity, but of order. A purely psychological statement of the resemblances and correlations between experiences must precede, not only every psychological theory regarding their connexion, but also, and *a fortiori* every physiological theory of their basis. It cannot be a safe proceeding to construct physiological theory by inference from psychological facts while the task of systematization of the psychological facts is neglected, whether a psychological theory of these facts is given or not.

#### IV. WERTHEIMER'S CRITICISMS OF CERTAIN PSYCHOLOGICAL THEORIES.

In Wertheimer's experiments, as in Wohlgemuth's, motion is seen when there is no real motion at all, but only the successive appearance, at times separated by varying intervals, of (usually) two brief stationary visual stimuli, *a* and *b*, separated by a short space, or at right angles to one another, like the two parts of the letter *L*. The

motion seen may be indistinguishable from the seen motion of a single real object, *e.g.* a short line turning through a right angle, or it may be double, as if first one small line made a movement through say  $30^\circ$  downwards from the vertical and then another small line through say  $30^\circ$  into the horizontal position; or it may even sometimes be so evanescent as to appear, apart from the motion of either small line, as a sort of pure, abstracted motion in a definite part of the field between the two lines, a mere 'going over' or torsion. All this, moreover, withstands the test of incognitive methods, just as does the examination of pictures shewn by the cinematograph. One may know the theory of the cinematograph or not, it makes no difference. So here again, the mind does not play a part in the production of the motion, not even by associating the parts omitted by the cinematograph with the parts shewn by it. For the motion will be seen even for objects that have never been actually seen by the spectator, *e.g.* an aeroplane, just as well as for the most familiar objects. Besides how could reproduction of the lost stages make the pictures move when they are not in motion at all, so long as they can be apprehended by vision as pictures? Thus we seem again to be driven to the physiology of the central nervous system for an explanation.

No objection need be raised against Wertheimer's physiological theory from the psychological side. Evidently it is only a theory of this kind which, as Wertheimer shews, can explain the facts relating to the production of motion by the stroboscope, the cinematograph, or other similar devices. I wish only to call attention to his criticism of those theories which attempt to regard motion as a form-quality (*Gestaltqualität*) or as a complex quality (*Complexqualität*) or the like, and which attempt to construct a psychological theory of motion from this leading idea. In Wertheimer's view these theories are put out of court by the fact that they demand that the motion which arises when the stimulations *a* and *b* are given in the manner described, shall apply to, and embrace, phenomenally both *a* and *b*. But, as Wertheimer has shewn experimentally, there are such things as "partial movements,"—*a* moving over one space and *b* moving over another space, the two movements being separated from one another by a small space; there is also such a thing as singular movement, when only *a* or *b* moves; and, best of all, the seen motion may not apply to, or embrace, *a* or *b* at all: these may be completely at rest and there may be in the space between them the phenomenon of pure motion or torsion, an experience much like Wohlgemuth's after-effect.— Thus a theory which



suggests that motion is founded upon at least two contents, in this case *a* and *b*, may be dismissed without further comment. Besides such a theory would have to explain all the other facts gathered by Wertheimer, which, needless to say, it could never do<sup>1</sup>. Wertheimer, finally, offers a physiological theory of the facts, his theory of "physiological short-circuit."

It may very well be that this or that theory of the type criticized has, in its ignorance of the facts, attempted to explain what was known of the facts of stroboscopic movement by using *a* and *b* as "founding contents" (*fundierende Inhalte*). But a critic may be expected to see the virtue, as well as the vice, of a theory. Like the eastern monarch who was invited to witness a horse race, and replied: "I already know that one horse can run faster than another," may we not also say: "we know already that there can be two disconnected movements, or that one thing may move and another be at rest, or that a motion may take place in the space between two things without affecting either?" Surely if Wertheimer offers a physiological theory of his facts, he thereby discredits his criticism of the form-quality type of theory! A felt motion may have any manner of cause you please, so long as the felt motion is supposed to correspond to its subservient, central neural basis. In Wertheimer's experiments *a* and *b* are mere stimuli, not founding contents.

## V. THE PRESENT THEORETICAL OUTLOOK.

I indicated in the opening lines of this paper that neither Wohlgemuth nor Wertheimer explicitly discusses the general attitude he adopts towards psychological and physiological explanations. They do not say why a psychological theory need not be offered for certain facts, nor why a physiological theory of these facts is admissible. Probably the reader is supposed to be sufficiently disciplined in these matters already. But if the relations between the component parts of a complex attitude remain obscure, there is grave danger that one of these parts may be over-emphasized and overworked, so that confusion results. It will therefore be well to discuss this attitude, to clarify the relations of its parts, and to find which should dominate the others if the best and most harmonious results are to be obtained. *The whole situation may be seen analytically by means of a survey from two opposite points of inquiry.*

<sup>1</sup> Cf. Wertheimer, pp. 242 f.

*(a) What reasons can be given for the absence of psychological theory?*

There are three which may be imputed to these authors. Either (1) they feel convinced that there is nothing for such a theory to explain; or (2) they see in experience no basis upon which pure psychological theory might be built up; or (3) they are convinced that the facts of experience are mere discrete differences which can be explained only by physiological theory, based upon the special relations between experiences and the various features of the stimulative processes which evoke them. Acceptance of the third situation obviously excludes occupation of the first two. For if there is nothing to explain, there is no need for a theory of any kind; and if experiences are not connected in some way, but are mere discrete differences or qualities, the physiological entities (mechanisms, etc.) deduced therefrom will also be discrete and unconnected and therefore useless. And that is what we find; for just in so far as Wohlgemuth and Wertheimer identify pure motion or the after-effect with ordinary motion, they construct their physiological theories to accommodate both; and in so far as Wohlgemuth distinguishes motion from successive and continuous change of position<sup>1</sup>, he must be held to give a purely illusory theory of motion or he assumes the existence of what he calls a "subcortical centre of movement<sup>2</sup>," and that, after all, is nothing but a ready made, specially created machine, which cannot have evolved out of the fundamental neural processes. But surely both the body and the mind must evolve; and if so each must evolve out of its own fundamental processes by the inner necessity and illumination that is given by progressively increasing effectiveness. To treat experience as a heterogeneous collection of elementary varieties, more or less similar, but essentially independent, therefore renders every scientific endeavour based upon the study of experience nugatory. Experience, like the starry sky which guides the sailor, is not merely one of the happy accidents of creation, merely "just so," and no more. It was a world of life before the sceptics tried to take it as an occasional, natural chart to the dark oceans of neural physiology. And it will be all the better a chart when it has again taken its place in knowledge as an ordered, inwardly coherent world.

If then we neglect the systematization and theoretical study of experience, we upset the natural hierarchy of the component parts in the complex task of the psychologist or psychophysicist and so achieve confusion. Neglect to systematize experience leads to neglect to systematize the physiological mechanisms we imagine by inference.

<sup>1</sup> Wohlgemuth, p. 88.

<sup>2</sup> *op. cit.* p. 99.

And without systematization there can be no theory of the evolution either of the brain or of experience.

(b) Adopting an opposite point of inquiry, we may now ask: *What insight justifies the confidence with which a physiological theory of certain facts is offered and admitted?* When double contacts give single touch, anyone apparently may understand that that is explicable only by a theory which assumes the existence of a single point of maximum central excitation and explains the way in which that arises out of the given double peripheral excitations. When an after-effect of negative sign arises from preceding objective movement, or when a movement arises from one or more resting stimulations, anyone may likewise understand that no laws of mind lie hidden here. The assumption of an indubitable parallelism of mind and body seems to be the only justification of these views. But this assumption, as we have seen, is abandoned by Wohlgemuth in his special physiological theory of motion and its after-effect. Why should we, then, retain it at all? Why not maintain that, when single touch results from double contacts, both the central and the peripheral excitations are double, and that single touch is due to the fact that for the two excitations the soul has rendered only one experience? Alternatively we might assume that the two excitations really did arouse two sensations, but that these two fused for some reason into one. Such assumptions have indeed been made, not perhaps for double contacts, but for those binocular stimulations which result in single fused vision. And no charge of absurdity or of obvious error could be brought against them. But these two cases of single touch and single vision from double stimulations are essentially parallel in nature. For the former only physiological explanations are generally admitted; for the latter physiological reasons have also been given, but they have been held to be utterly inadequate and psychological interpretations have been favoured instead<sup>1</sup>. If the physiological or the psychological line of explanation is preferable in special cases, there must surely be clear ground for the preference.

This ground seems to me to be a tacit recognition of the possibility and validity of pure psychological theory. In dealing with single touch or any other similar sensations, we recognise that there is nothing psychologically simpler and more primitive than elementary sensation itself to which we might appeal for an explanation of its characteristics. Consequently, if one class of sensations shews features which another does not possess, we feel justified in assuming that the anomaly must

<sup>1</sup> Cf. W. McDougall, *Body and Mind*, London, 1911, chap. xxi.



be due to the peculiar nature of the stimulus or of the receptor of that sense, *i.e.* it must be due to physical or physiological causes. All sensations, then, must be of one psychological class and of one psychical type and must behave, apart from extraneous causes, in the same way. This assumption is quite admissible as a working hypothesis, since no positive arguments can be brought against it, no matter how difficult it may be to establish it. On the other hand, the admission that, apart from discrete differences in quality and in the extent of range of variation of any attribute, sensations may be of different types, is scientifically self-destructive. For the departure from type means the failure of generalisation and therefore the absence of explanation. There can be no true science of psychology at all, unless the simplest sensations conform essentially to one type. Hence the common appeal from the psychology of the sensations to physiological theory implies both the admission of the assumption of types and the recognition of a fragment of pure psychology.

Similarly it is justifiable to offer a physiological theory of the after-effect of seen movement and of stroboscopic movement; for there seems to be no obvious psychical reason why the after-effect should be of a direction opposite to that of the preceding objective movement. If previous writers have offered psychological theories, a closer examination of the facts shews that the processes they appealed to are not involved in these experiences<sup>1</sup>. Nor is there any apparent psychical reason why the presentation of a successive series of stimuli differing in position should arouse the experience of a continuous movement over a distance or of many small neighbouring movements, etc. Besides, these peculiar effects are so like the experiences evoked by objective movements that we may at once assume that the physiological basis of the latter is identical with that of the former. Psychological theory has, then, only to classify and systematize the varieties of movement experience and to set them into relations of resemblance to the already classified simple sensations. The result of this task defines the problem for the physiological theory of motion, which has not only to imagine a neural basis of motion, but has also to shew how it is connected with the neural basis of the simple sensations, besides indicating, by reference to the incidental features of the physical processes taking place in these neural structures, how the anomalies of the correlation of external or preceding stimuli and consequent experiences (reversed after-effect, movement from stationary stimuli) are to be accounted for.

<sup>1</sup> Cf. Wohlgemuth, pp. 90 f. ; Wertheimer, pp. 240 f.

This attitude towards the problems of simple sensations and the simplest other sensory experiences is confirmed by a consideration of those cases in which two systems of sense-organs, eyes or ears, work together to make certain experiences possible. A careful survey of the problems is here made inevitable, because the facts suggest the view that the unity of binocular vision has no unitary neural counterpart<sup>1</sup>. We seem compelled to allow that we get unitary vision not only from double peripheral, but also from double central excitations. That the method of approaching these cases must also give *first place to positive psychological classification and theory* I have attempted to shew elsewhere<sup>2</sup>.

<sup>1</sup> Cf. McDougall, *loc. cit.*

<sup>2</sup> "The Relation of Mind and Body," this *Journal*, 1912, v. 299 ff.

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# THE MAIN PRINCIPLES OF SENSORY INTEGRATION.

BY

HENRY J. WATT.

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## THE MAIN PRINCIPLES OF SENSORY INTEGRATION<sup>1</sup>.

BY HENRY J. WATT.

- A. 1. *The systematization of the sensations.*
  - a. *Note on extensivity.*
- 2. *The systematization of the integrative sensory modes.*
  - a. *Note on the word 'mode.'*
  - b. *Note on the word 'integration.'*
- B. *The main principles of integration.*
  - 1. *"The mode which results from the integration of an attribute must bear an immediate introspective resemblance to it."*
  - 2. *"The results of the integration of the same generic attribute in the different senses must be introspectively and functionally similar."*
    - a. *The sub-principle of the explanation of apparent exceptions and limitations to this rule: "If a mode of experience does not occur where we might for any reason expect it, this can be explained only by the absence of the variant experiences upon which it is integratively dependent and for the latter the natural limitations of physical and physiological processes must be ultimately responsible."*
  - 3. *"Every typical mode of experience must to some extent at least arise spontaneously and automatically and independently of such processes as will, attention, inference, proof, or the like."*
- C. *Conclusion.*

THE formulation of principles is an important stage in the advance of any science. Its beneficial effects far outweigh its disadvantages and

<sup>1</sup> An abstract of this paper was read before the Sub-section (to Section I) of Psychology at the Meeting of the British Association at Birmingham, 1913.



dangers. It is the sign of an increasing unanimity, a concentration of criticism in various fields round one or two points of view, a growing sense of the inherent connexions of the subject-matter. It means the abandonment of extraneous principles of explanation most successful, it may be, in objectively neighbouring provinces of science, but really inapplicable to the one under consideration. It serves, moreover, as a guide to research and to theory, thus supplementing mere exhaustiveness by some degree of enlightenment. And it is perfectly safe, unless it is the outcome of a movement towards prejudice and bias.

The formulation of principles is highly necessary in psychology, for it is recognised by many to be a sphere in which the effects of the interaction of all the main forms of being—physical, physiological, biological, psychical, and social—are made patent. The introduction of extraneous principles of explanation is highly probable in this case, unless sufficient attention be given to the nature and applicability of the principles to be admitted. The principles of the natural and biological world do, of course, make themselves felt in the sphere of experience. But they do not provide a sufficient basis for the proper systematization of that sphere. The peculiar nature of the psychical itself must be emphasized and principles must be devised for its elucidation which are drawn from its own sources and may therefore be expected to do the only full justice to its particular difficulties. This claim is, in fact, an assertion of the priority of the psychical in the psychical realm. It is also an assertion of the possibility and necessity of a purely psychological systematization of the psychical.

A. A systematic psychology of sensory experience is perhaps the greatest need of our science at the present time. It has been very much neglected. That, no doubt, is due to the fact that the chief motive of the study of the senses has been physiological. There seemed to be so much to be gained by this physiological study and so little air to breathe in a purely psychological atmosphere. But surely there is no use in talking of a science of psychology at all, unless the realm of sensory experience can be properly systematized. The simplest and most fundamental problems involved in this task fall into two main groups.

1. *The systematization of the sensations* is the first of these. Some sort of a 'periodic table' of the sensations must be formed, which will serve as a framework and basis for any theory regarding the qualities of sensation; and the attributes of sensation must be reduced to a type. This psychological task is a necessary preliminary to any pure psychology

of the senses. I have attempted to fulfil it elsewhere<sup>1</sup>. Only a short summary and revision of the outcome of that attempt need here be given. Of the six attributes of sensation, quality and intensity stand somewhat apart from the others. Quality may be considered to occur only in single and discrete forms in all cases, except in the senses of vision and smell whose purely psychological treatment is still problematical. Hardly in any case is there any dispute or difficulty concerning intensity. The four other attributes—of extensity, order, duration, and position in time—may be arranged usefully in the following scheme:

Generic names of the various dimensions of sensation	Generic names of the attributes :	
	Extensity	Order
	These are inherent	
	WITHOUT	WITH
	variation, in all sensory	
(Intra-) Systemic	extents, masses, volumes	localisations, positions, pitches
Temporal	durations	positions-in-time

It is tempting to bring quality and intensity into parallel with this scheme, so as to reduce the six attributes of sensation to a triad of pairs, each pair being extensive and ordinal in its own peculiar dimension. But this is impossible for the following reasons<sup>2</sup>:

(1) Qualities cannot be treated as orders, for they give no distances or motions; even if that fact be ignored, it is introspectively evident that they do not bear the stamp of an ordinal attribute. Even the different colours we do not think of as points in a system; how much less then do we consider the qualities of the different senses in this

<sup>1</sup> "The Elements of Experience and their Integration; or Modalism," this *Journal*, 1911, iv. 135 ff., esp. 148 ff. *Psychology*, London, T. C. and E. C. Jack, 1913, 21 f. Cf. "The Psychology of Visual Motion," this *Journal*, 1913, vi. 26 f.

<sup>2</sup> Cf. my paper "Are the intensity differences of sensations quantitative?" This *Journal*, 1913, vi. 176 f.

way. And if quality is not an ordinal attribute, it is certainly not a merely extensive attribute.

(2) Intensity, likewise, can be treated, neither as an extensive attribute, for it is essentially variable and is not introspectively identifiable with the extensive form of attribute; nor as an ordinal attribute, for it is neither phenomenally nor functionally like one of these.

(3) If quality and intensity formed such a pair of attributes, they should prove readily adaptable to quantitative purposes, as do the attributes of extensity and order in combination with one another in connexion with the measurement of space and time. But this is not the case.

*a. Note on Extensity.* The critical point of any discussion of this attribute lies in the problem of its relation to the attribute of order. When extensity is present in a pure form, according to Stout, as in the case of the voluminousness of sounds, "it has no distinctively spatial character, no internal order of positions and distances<sup>1</sup>." It seems as if the quantitative aspect of space could exist without a spatial order<sup>2</sup>. Such statements suggest the following question, which may be expressed in various forms: Is extensity as an attribute really variable? Has it for example, a minimum, say the sensory 'spot'? Or we might ask: Is the extensity of the minimum different in variety or amount from that of a postage stamp? Is the voluminosity of a high tone different in variety or amount from that of a low tone? Surely it must seem absurd to suggest assent to these questions.

What, then, are we to understand by the differences referred to, *e.g.* the 'vast discomfort of a colic or lumbago,' the peculiarities of high tones and of low tones, the differences of the areas felt from the contact of a pencil point and of a postage stamp? If extensity and massiveness and voluminosity do not differ, extents and masses and volumes surely do; these are the things we distinguish in these cases. But obviously no part is played in their composition by quality or by intensity, not to mention the temporal attributes. The only other attribute besides these and extensity is order, which does vary.

We may, therefore, suppose that extents and masses and volumes of sensation differ in virtue of the varying number of orders included within them (or by the varying number of sense-organs of neighbouring

<sup>1</sup> G. F. Stout, *Manual of Psychology*, 1899, 337.

<sup>2</sup> *Op. cit.* p. 334. Cf. also p. 336: "We have all kinds of gradations between pure extensity and fully definite extension." "Typical cases of extensive diffuseness or massiveness are afforded by organic sensations" (p. 337).



position that have been excited). This conclusion is quite consistent with the psychology and the physiology of the cutaneous, gustatory, and visual sensations. Hesitation can only arise in connexion with the massive sensations, articular, muscular, organic, and auditory. But it must yield to a reiteration of the priority of psychological systematization and of the probable conformity of the results of physiological study thereto. If muscular sensations from muscles of different size, and articular sensations from joints of different size, differ in massiveness, surely there need be no hesitation in supposing that this difference is correlated with a difference in the number of receptors excited. The same remark applies to the sensations of colic, lumbago, hunger, thirst, and the like. The varying voluminosity of sounds suggests that each sound is really a mass or extent of sounds; high tones are thin and short, low tones are longer and perhaps bulkier, and, it may be, more tenuous as well. Such a view would explain why the pitch and the voluminosity of tones are fixedly correlated with one another. It is the psychological statement to which Ewald's theory of hearing<sup>1</sup> in many respects forms a most suitable physiological counterpart.

But although extensity is not variable, it is a true attribute of sensation, readily distinguishable from order. Without it we should have neither areas nor voluminosities. That is evident if we remember that a cognitive form of order<sup>2</sup> exists to which there is no accompanying extensity, so that it is impossible to make a series of concepts, such as those of number, adequately represent the real continuity of an objective line or area. It might be supposed to be a sort of sensory stuff, which is repeated and multiplied by the repetition of orders. But the same notion would apply equally to any of the other attributes. The quantitative treatment of extents and durations is possible, only in virtue of the close, psychical kinship between sensory orders and conceptual orders; in a certain respect the latter grow immediately out of the former, although they are extended very much beyond the range of the variations of sensory order. Measured extents are not measured extensities at all; for, as we have seen, extensity is not varied. But extensity can be involved indifferently in a statement of what is measured, because it is itself unvaried and can, therefore, introduce no confusion or complication into the comprehension of that statement. Extensity, for the same reason, seems to have a minimum only in relation to order. A distinction of orders within the 'spot' is, of

<sup>1</sup> J. R. Ewald, *Arch. f. d. ges. Physiol.* 1899, LXXVI. 147 ff.

<sup>2</sup> Cf. K. Bühler, *Arch. f. d. ges. Psychol.* 1907, ix. 357 f.

course, thinkable, but it does not exist in sensation. So extensity seems to be variable only in conjunction with orders, especially when the latter are all continuously adjacent and are given along with uniform quality and intensity. Then the fusional function of extensity comes into action and we get continuous extent or area. But the differing orders involved in this extent, though no longer separately distinguishable, are effectively present. It is just they which determine the extent of the sensational area or mass.

If orders are to be separately distinguishable under areal or massive conditions, they must evidently be accompanied by variation in some other attribute. The only other variable attributes are quality, intensity, and position in time; but there may be variation in more than one of these at the same time, of course. This consideration seems to be of some importance for the theory of orders and their complications<sup>1</sup>.

It must be obvious that the above statements apply equally to the attribute of duration. It is essentially an unvaried attribute, which gives variable durations or stretches of time only in conjunction with the variable attribute of position in time.

2. *The systematization of the integrative modes* of sensory experience is the task that for a scientific psychology inevitably follows upon the systematization of the simplest sensations. In so far as these modes occur under different circumstances, they must be identified and reduced to types of graded complexity and referred to their typical conditions, so as to come within the purview of a general, systematic theory of the constitution and interconnexions of experiences. Of these modes there are two main groups—those which take place between sensations which belong essentially to the same sensory system and those which take place between sensory experiences which, like those of the two eyes or the two ears, belong to different systems. Of the former, intrasystemic integrations, distance, and interval of time are the simplest. In many cases they involve a difference in the sensations which make up the distance or the interval of time only in respect of the attribute of order or of position in time; and in those cases in which a variation in extent or in duration is noticeable without any accompanying discreteness or separateness of sensations in respect of order or of position in time, we are justified by consideration of the circumstances of stimulation in extending our statement and in assuming that, in these cases also, distance and interval of time are based upon sensations which differ only in respect of the attribute

<sup>1</sup> Cf. my discussion of "The Psychology of Visual Motion," this *Journal*, 1913, vi. 26 ff.

of order or of position in time. Moreover, distance occurs only in those senses whose sensations differ readily and obviously in the attribute of order. We are never called upon to distinguish hunger or thirst distances, or distances of muscular sensation, or smell distances. In these senses the variation that we notice is at most one of extent or of massiveness. In so far as distance occurs in different senses, however, we must expect and do find that it is phenomenally and functionally the same.

All experiences are qualified by position in time of some form; consequently we can experience an interval of time between any two experiences. But the interval is distincter when it is constituted by experiences belonging to the same sense, and still more so when it is given in those senses which are specially rhythmical, namely sound, vision, and the motor group of senses—the articular, the muscular, and the tactual. In these senses the stimulus can be readily manipulated so as to cause an experience to begin and to cease at any desired moment.

Distance and interval of time are, as modes of sensory experience, peculiarly simple, in that they are the only modes which necessarily involve a variation in only one of the attributes of the sensations upon which they are, or may legitimately be supposed to be, dependent. On the other hand, order and position in time are themselves the only two attributes of sensation which can vary apart from variation of any of the other attributes of sensation. Thus analysis confirms the introspective simplicity of these modes.

The sensory mode that stands next to these two in point of simplicity is motion. For many reasons it may be considered to be a combination of the modes of distance and of interval of time. It is therefore found in those senses which present the mode of distance. Its phenomenal and functional identity in these senses, especially in that of sound where it forms a part of what is collectively called melody, is a problem of great interest at the present time. But the study of motion presents peculiar difficulties<sup>1</sup>. For the present it may suffice to say that motion is a combination of the two modes of distance and of interval of time, involving simultaneous and continuous, though not necessarily concomitant, variations in the attributes of order and of position in time of the sensations which integrate to form it<sup>2</sup>.

<sup>1</sup> Cf. "The Psychology of Visual Motion," this *Journal*, 1913, vi. 26 ff.

<sup>2</sup> For preliminary work towards the systematization of the modes of distance and of motion, see my paper in this *Journal*, iv. 172 ff. and 157 ff.



a. *Note on the word 'mode.'* I find the use of this word very convenient<sup>1</sup>. It serves, of course, in the first place to distinguish those experiences which we may legitimately suppose to be integrated out of simpler experiences, from experiences such as the simplest sensations which show no sign of such derivation. But if we may presuppose the systematic classification of these modes, we can then with the help of this word and of adjectives signifying the name of each class of modes indicate without any ambiguity or confusion exactly the kind or complexity of experience involved in any particular state of mind. That cannot be done with the commonly used word 'perception.' When we speak of the perception of distance, it is not clear what exactly is meant. Do we mean the perception of distance as an object for the mind or as an experience, or do we merely mean the presence and effectiveness of distance in our sensory experience? If we wish to study perception as distinct from any sensation or sensory mode, we can indicate that by speaking of the study of the perceptual modes of experience.

The word 'mode' will also translate the German word *Vorstellung* in many of its uses, for example in its application to the term *Gestalt*, which has been used to indicate distance and motion and many other experiences which differ from sensation in the same way as these do. But it can only be misleading to talk of the 'quality' of a mode or *Gestalt*. Every mode has its own introspective nature and affinities, but these have only seldom anything to do with quality. Although the unqualified use of the word 'mode' well translates the unqualified use of the word *Vorstellung*, the use of the latter word is apt to be as misleading as the English word perception, e.g. when we read in one sentence of the *Vorstellung der Zahl*, *Vorstellung der Distanz*, *Vorstellung der Aehnlichkeit*, and *Vorstellung der Verschiedenheit*<sup>2</sup>. There are such things as sensory number and difference, but they are surely not modes, the same things as are distance and motion; there is a sensory mode of distance and a conceptual mode of distance, but there is a great difference between them. We proceed unscientifically if we lose sight of these differences.

b. *Note on the word 'integration.'* This word indicates that the resulting mode unifies the sensations to which it refers and is attached and upon which it is psychically, if not also psycho-physically, dependent. The word may therefore be used generally to express the known relations

<sup>1</sup> Cf. this *Journal*, iv. 203; *Psychology*, 1913, chaps. II. IV.

<sup>2</sup> E.g. Witasek, *Grundlinien der Psychologie*, 1908, 222 ff.

between modes of experience and the simpler experiences upon which they rest. And an inductive study of these relations in various cases may be expected to lead us on to knowledge we could not gather from any one particular case. So the word integration may imply the general theory of the relation of a mode to its basis in experience, which psychology may hope some day to attain. If this is borne in mind, the use of the word can make neither for obscurity nor for confusion, but can only be the means of scientific concentration and inquiry.

B. After these preliminary statements we may now consider the main principles of sensory integration.

1. The first principle is as follows: *The mode which results from the integration of an attribute must bear an immediate introspective resemblance to it*<sup>1</sup>. Or: Among the attributes or features of the simpler experiences upon which a mode of experience is, or may legitimately be supposed to be, psychically dependent, there must be one to which it bears a much greater introspective resemblance or affinity than to any other. The latter statement is more inductive in outlook, while the former is more deductive. Only on the basis of such a principle as this can a theory of psychical derivation or causality be built up which will reveal in the world of mind that rationality and intelligibility which we naturally expect to find in all things. The position involved in this principle has been reached by psychology in three distinct steps.

a. For each variation in the derived or integrated state analysis and experiment must show an unambiguous complex of stimulatory or sensory data. This is an obvious and uncontestable truth. Only about the relation of the derived state to the experiences with which it is objectively correlated can there be any dispute.

b. Either: we talk in all cases only of stimulatory data, no matter what the experiences we are investigating may be, mere aggregations or unique modes. This position is taken by very many psychologists of the present time. It leaves, of course, no room for the principle stated above; but neither does it leave any room for a science of pure psychology. All we can then expect is a mere distinction of mental states from one another and a correlation of them with *physical or physiological data, that is to say, psycho-physics or psycho-physiology*. An inquirer of a logical turn of mind might well ask how we can have mere distinction without some trace of interconnexion by resemblance, and, thereafter, without some theory in explanation of this resemblance; but if this thought arises in the minds of those who remain at the

<sup>1</sup> Cf. my *Psychology*, p. 26.

position of this paragraph, it is rendered ineffective by some indefinite belief which makes any hope of constructing a reasonable explanation of the merely similar, or generally of the psychical, untenable. It must, of course, be obvious that if there can be no pure psychology of sensory experience, there can be no pure psychology of any kind of experience at all.

Or: we allow a resultance of certain experiences from others by association or by 'experience,' while denying the principle under discussion. This position is closely associated with the theory of local signs, but it is also in vogue with many in the treatment of cognitive and other experiences. But it must be clear that the effect of experience is unintelligible and association is impossible unless each of the associating elements already differs from every other, whether it be by its locality or order, or by its place in experience, or what not. A series of identicals cannot be differentiated by any association with a series of variants, if that association operates from the identical elements towards the variants. To allow this would be to deny the truth of the rule stated under (a) above. This alternative position, then, allows of a pure psychology, in the sense of a system of correlations of an objective kind between single experiences or between groups of experiences. But it blocks the prospect of an intelligible and reasonable science of experience. We must look for a corrective to its negative attitude in further insight into the origin and nature of association.

c. Association cannot be mere blind mechanism, a sort of bond that arises when experiences impinge upon one another in the mind and that requires no sort of counterpart or basis of origin in the experiences that become associated. The purely mechanical view of association prevails at the present time in the treatment of memory; for association can be treated systematically from a mechanical point of view. But this abstract theoretical procedure may be only a part of the whole truth. Purely mechanical memory involves the assumption that experiences associate when they come into contact in the mind in complete indifference to the affinity or dissimilarity of their 'contents.' The most reasonable constellation of ideas, then, has a greater coherence than any other grouping only because there are in it a greater number of frequently repeated and therefore strong associations. Meaning is just a general convergence of associations. But this is surely not confirmed by the facts. What is associated must surely cohere as conscious experience before the association arises. Of course



there must first be contiguity of a certain degree between the associating parts; they must occur within a certain stretch of time. But must we not suppose that having thus occurred they cohere because of their psychical affinity, and that having cohered and integrated they can then become associated to one another so that the one can revive the other? Mere mechanical memory means mental chaos and irrationality. Fortuitous contiguity would as easily produce a coherent mind, as fortuitous grouping of elements and natural selection would produce the biological world without the coherent basis of law given in the physical and chemical world. "A unitary mode of experience in which the associating experiences are integrated is always presupposed, although it is usually ignored<sup>1</sup>."

This principle is the outcome of all unsuccessful attempts to derive special experiences from the grouping of other kinds of experience with the help of association alone. Neither local sign, nor stereoscopic vision, nor perception, nor the concept, nor recognition, nor thought, nor any other unique and special kind of experience, can be satisfactorily explained in this way. And if we must return to a direct consideration of the basis of coherence or of integration in the introspective nature of the experiences that form the basis of integration in all these cases, must we not also look for an integrative basis in experience even in the case of the seemingly most mechanical of associations? We may be in doubt about thus generalising the result, but there can be no hesitation about accepting the principle in the case of all unique modes of experience. If the objective dependence of one experience upon others compels us to classify it as a special mode of experience, and if we may therefore hope for a theory of its derivation or integration out of some one or more features of the experiences it is psychically dependent upon, then it is clear that we can look for its integrative basis only among those features of the experiences upon which it is dependent which bear an introspective resemblance to the mode in question. The true basis of integration will bear a greater resemblance to the mode in question than any other feature of the integrating experiences. It is evident that such a principle will serve as a guide both to experimental research and to theory. Moreover, if a mode is variable, the components of its integrative basis must be variable, as in the cases of distance and feeling; but if it is invariable, as in the case of recognition, the components of its integrative basis cannot be variable.

Whatever is, is rational. In reference to the present position

<sup>1</sup> *Psychology*, p. 60. Cf. this *Journal*, iv. 130, 139, and esp. 149 f.

of integrative psychological theory, this means that if we are to suppose that dependent mental states are derived from the integration of those upon which they are dependent, it would seem to us more satisfactory and intelligible that there should be some degree, or the highest possible degree, of resemblance between the dependent state and the feature or attribute of the conditioning experiences upon which the former in the case of variable modes is known to be dependent and in the case of invariable modes may be supposed to be dependent. More than this we cannot expect. If unique types of experience do not bear quantitative relations to one another, the relations that exist between them cannot in all cases be those of the type of reasoning. For that would be a denial of their specific nature. A standard for the discovery of these relations can then be found only in some other general appeal which the typical form of these relations in known cases may make to our minds. One element in that appeal at least must be degree of resemblance between integrative basis and derived mode. What other elements it may contain inductive research will show. Only on these lines can we hope for a science of pure psychology.

2. The second principle of integration is as follows: *The results of the integration of the same generic attribute in the different senses must be introspectively and functionally similar*<sup>1</sup>. Stated more generally it reads: the introspective and functional nature of an integrated mode of experience is essentially independent of the attributive or other accompaniments of its integrative basis. Wherever the requisite integrative basis occurs, the same generic mode will result. This principle is a necessary step in the systematization which is to constitute a pure psychological science. I have attempted to establish it in detail in the case of the simplest sensory modes of distance and motion<sup>2</sup>. But it must also hold in such cases as feeling, recognition, thought, and the like, for these can be occasioned by the most varied sensory and other experiences. The integrative bases of any mode must be considered to be the same in all cases, no matter what the accompanying differences may be. Experimental research will undoubtedly lead to the confirmation of this principle in all accessible cases. Very often the similarities of modes are passed by as mere analogies. That may serve as a good maxim where there is no insight into the systematic nature of experience to act as a guide. But it would be wrong to block the outlook

<sup>1</sup> Cf. my *Psychology*, p. 27.

<sup>2</sup> See this *Journal*, iv. 157 ff.

and progress of systematization by an ascetic cult of this idea of analogy.

If this principle be granted, we can hope to establish general rules for the relation of generic modes to the generic attributes or features of the experiences from which they are integrated. For example, "motion is found developed upon every group of sensations which show distinct variations from one another in order<sup>1</sup>"; and, "we find distance in all those senses which show order and are capable of the modification of motion<sup>2</sup>." Rules may also be expected to hold for the limits of time within which alone the integration of those modes that are based upon successive experiences can take place. For we have reason to believe that in so far as all experiences are qualified by the attribute of temporal order, all integrative processes which involve successive experiences are subject to certain limits of difference of temporal order.

This principle would also lead us to expect that if a certain mode of experience can be integrated from simultaneous components it should also result from the integration of components which follow one another within the time limits just mentioned. Conversely we should be able to transfer our expectation in a similar manner from successive to simultaneous integration of the same mode, unless, of course, differences in either of the temporal attributes be an essential part of the foundation of its integration, as is the case in the integration of motion. If the temporal attributes are not the essential basis of an integration, it is clear that any differences in them that fall within the time limits of integration, should be as irrelevant to the integration as is the presence of identical or unvaried attributes.

It cannot, of course, be evident in detail how far this irrelevance of accompanying differences, such, for example, as those of quality in the case of distance and motion, extends. But it is assured by a broad consideration of the conditions of occurrence of the various experiences hitherto distinguished by psychology. We must therefore be on the look-out for it; and if it is not forthcoming as we should expect, we must find good objective reasons for its absence. It is fortunate that in the finding of these good reasons we can accept the guidance of a minor principle of explanation.

a. *The sub-principle of the explanation of apparent exceptions to this law.* If a mode of experience does not occur where we might for any reason expect it, that can be explained only by the absence of the variant experiences upon which it is integratively dependent and for

<sup>1</sup> This *Journal*, iv. 157.

<sup>2</sup> *Ibid.* 173.



this the natural limitations of physical and physiological processes must be ultimately responsible.

It is the task of science to expound with the utmost detail the nature of the coherence that binds events into unitary systems of greater and greater extent. Each particular science is concerned with a part of the whole that more or less obviously forms a unitary system. If it discovers in its sphere that kind of coherence that characterizes another sphere of science, it thereby joins with that other to form a system of greater extent than either. But it does not therefore identify its subject-matter wholly with that of the cognate science. The two remain distinct in so far as the forms of coherence that characterize them differ. Now no one would deny that the forms of coherence that characterize the psychical world differ very much from those that characterize the physical and the biological worlds. But they are not wholly independent; something is common to them all. For on any view whatsoever it is clear that our knowledge of the physical world is dependent, not only upon the actual occurrence of physical processes, but also upon the transmission of these in some form or other through the sense-organs to the central nervous system. We can know of a physical process only if the differences of the parts and the manner of the arrangement it involves can be brought into correlation with those involved in a unitary psychical process. This holds, not only for cognition, but also for any kind of adaptation that may exist between the physical and the psychical realms. Such adaptation can occur only in so far as by some means or other a correlation of process can be carried through the three kingdoms of the physical, the physiological, and the psychical. In so far as physical processes occur at a slower rate of change than the minimum required for psychical integration, we cannot become aware of them, unless we can secure some means of bringing their rate of change within the narrow compass of the mind. If a physical change cannot be made to affect a physiological organ appropriately, we must remain ignorant of it, unless we transfer it through some medium which we understand so as to obtain the appropriate effect. And so on.

The mode of distance, for example, cannot be produced apart from variation of the attribute of order; it is therefore practically absent from the organic, muscular, and olfactory senses. In the organic senses there may be a certain variation in massiveness, involving difference of orders, but we do not have a hunger distance or a thirst distance in any proper sense of the word. Similarly we notice that the muscular

sensations from different muscles differ in massiveness and are localised at different parts of the body, but the sensations that come from one and the same muscle do not seem to vary in massiveness or in localisation. Thus a muscular distance, which might be constituted by the simultaneous occurrence of sensations from different muscles can hardly occur without the simultaneous excitation of such tactual sensations as would form a tactual distance. The latter for various reasons, such as variation, frequency, and correlation with other senses and modes, have a cognitive value that the former can never acquire for want of variability. Muscular distance will therefore be so obscure or so blended with tactual distance as to be hardly noticeable. In the sense of smell, distance seems to be quite lacking. If there is any olfactory order or localisation it seems to be so unvaried as to be useless. And even if smell has its order in some other form than localisation, in us at least the sense is so sluggish that the variations of order necessary for distance cannot occur within the time limits of integration. The same reasons as prevent the occurrence of distance prevent *pari passu* the integration of motion.

Interval of time is found under all possible circumstances, in all regions of experience. Only in the form of rhythm is there any restriction to its occurrence. The reason for that fact has been already mentioned: only certain experiences can be made to begin and to cease at any desired moment or periodically. So we cannot have rhythms of taste, temperature, smell, organic sensation, feelings, ideas or thoughts.

The peculiar correlation which is found in the sense of sound between pitch and voluminosity is responsible for all the limitations of integration which specially characterize this sense. Pitch is an aspect of sound which represents the individuality of the sounding object much better than does its spatial localisation. Besides, it seems clear that if the latter had been maintained at all costs on the basis of simple sensation as a sort of local sign, the former would never have been developed. The greater advantage lay in the attainment of a discrimination of pitch even at the temporary or permanent sacrifice of a direct auditory form of localisation. But two more or less efficient methods of localisation have been secured—the mobile-ear-funnel method of many animals and the binaural method of man. As a consequence, however, of the preferential development of pitch we have no true experience of auditory solidity and the smaller variations of tonal interval are rendered highly unclear or even impossible by the presence of beats and intertones.

It is of interest in this connexion to recall a remark made by Ewald. He wrote<sup>1</sup>: "Man begeht immer gewisse Fehler wenn man die Funktionsweise eines Sinnesorganes mit der eines anderen vergleicht." "Wenn der physikalische Anlass für eine bestimmte Empfindung sich in irgend welcher Weise ändert und dadurch eine Veränderung der Empfindung bewirkt, so scheint mir keine Uebereinstimmung im Wesen der beiden Veränderungen bestehen zu müssen." But this is a principle of apology which cannot be accepted from Ewald. For the merit of his theory, apart from its experimental foundation,—a merit that is brought forward into the light by his own sixth argument against Helmholtz's theory—is the facility with which the phylogenetic development of hearing can be traced with its help. For it is just because and in so far as the physical variants of sound have always been the same and the physiological apparatus they play upon has gradually changed in the course of the development of the race, that the psychical results have gradually developed. The peculiar nature of the physiological apparatus has secured for it, not a fragmentarily specialised development, but an equalised development. The system of sounds which results is just as equalised and balanced in its nature. Besides, Ewald does assume that there must be some agreement between physiological and psychical changes; for he postulates a special physiological means of getting round the necessity for this agreement in the case of the ear:—his coupled-buttons theory<sup>2</sup>. This, however, is a forced and artificial way of overcoming his chief difficulty, which is to explain why, on his theory, we do not hear a series of identical tones for each component of a tone picture, instead of only one tone. In the light of his criticism of Helmholtz's theory, this part of Ewald's theory is just as fantastic as is Helmholtz's. For what *deus ex machina* is to make all these coupled-buttons-connexions for the organism? How are they to begin and to be progressively developed?

If we can once decide in what manner any mode of experience varies, we thereby obtain an index to the integrative basis of that mode. This guidance is of great importance in those cases in which the integrative basis of a mode stands in a complex psychological environment from which it is not easily distinguished or isolated. If the variation of a mode is restricted or if there is none at all, its integrative basis should consist of only one pair of unchangingly different experiences. Such a case may perhaps be exemplified by the mode of recognition.

<sup>1</sup> *Op. cit.* 181 f.

<sup>2</sup> *Op. cit.* 183 f.



A problem of considerable magnitude is presented in the case of the absence from certain minds of experiences known to other minds. Animals, for example, do not reason. Probably they also lack the general concept and all those cognitive experiences which involve it; they can hardly be supposed to localise their memorial experiences in their past. With all other simpler experiences they may well be presumed to be equipped. But if they can see and hear and smell and feel as well as we can, perhaps in varying ways better, why does their experience not develop upon this sensory basis to the heights it reaches in the human mind? The answer to be deduced from the principle here stated denies that the animal possesses the full integrative basis of the experiences it lacks. It would be presumptuous in the present state of knowledge regarding the higher cognitive states to attempt to indicate what is lacking or why it is lacking. An alternative view refers the limitation to restrictions set by the level of development that the brain of the animal has reached. But that explanation is either psychically blank and valueless, or it implies that a further development would add some experiences to those the animal already has and so make the appearance of the higher cognitive states possible. Thus either the view stated above is conceded, or it is assumed that the higher modes of experience come into being by direct dependence on the development of the brain, not through the medium of the simpler experiences of whose integration the modes in question may legitimately be supposed to be the result. On the alternative view a pure science of psychology is, of course, impossible. Such a conclusion can hardly be entertained seriously for long, whatever divergence of views there may be regarding the kind of elementary experiences that are lacking in the animal.

3. The third principle of integration is as follows: *Every typical mode of experience must to some extent at least arise spontaneously and automatically and independently of such processes as reason, thought, determining purpose, and the like, unless these processes themselves are the modes in question.*

If it be borne in mind that a mere aggregation of experiences presents no problem and that every mode of experience worthy of that name must make some new addition to experience, it might hardly seem necessary to state this principle explicitly. It might seem so obvious as to be trite. But much of the past and current theory of the growth and development of the mind so thoroughly ignores the problem of the unique modes of experience that the principle may seem

to contain a new and startling truth. There can be no universal guide to the development of the mind, be it called reason or thinking or self-realisation or teleology, or what not. The mind must develop when it can, when the conditions for that development have been given; and what then happens is really development, a step forwards, something new, no mere unmasking of the obscure. The only guide to mental development, if it can be properly called by that name, is the illumination each step of integration brings with itself. It is itself its own coherence and justification. It reveals its own necessity, in part at least, when it comes; but it cannot be foreseen. In the light of the preceding two principles, integrative processes are most reasonable and intelligible, and with increasing knowledge they will appear still more so. But they are not themselves the product of reasoning; they must arise spontaneously. It is important to emphasize this in view of the fact that thought and purposive determination and such other processes are not only the instruments of science, but are themselves also modes of experience which must arise spontaneously. As the instruments of science, reason and thought provide us with standards of coherence in the form of identity and repetition, approximation and similarity, and these are our favourite tests for the manifold forms of coherence we find in the various spheres of being, including the relations of modes to their integrative basis. But while retaining these tests even in these last cases, we must not lose sight of the fact that each unique integrative process is and remains unique, and therefore contains a justification of its own, which we can never hope to extract from it by any inductive or other cognitive procedure. That justification is simply the coherence and insight the integrative process itself is.

The higher cognitive and the conative processes bear another important relation to the integrative processes in that they may serve to extend the conditions under which they take place, to support them by making these conditions more enduring, more compatible with the limitations of integration, and therefore virtually wider in scope. Once an integrative process has occurred, its signs or criteria can be established for indirect use. "But unless our minds recognised, or thought, or felt spontaneously, we could never even begin to collect tests for the recurrence of experiences, or for the truth or falsehood of asserted relations or for the justification of beauty. Nothing but the direct insight of experience can set the mind the larger task of extending that insight to the uttermost bounds of reason<sup>1</sup>."

<sup>1</sup> See my *Psychology*, p. 27.

In the case of certain *nova* of experience it is relatively easy to show that they are integrative modes, but it may be very difficult to show from what features of the experiences upon which they are, or may legitimately be supposed to be, dependent in an objective psychical sense they are integrated. This difficulty may be supposed to be due partly to the complexity of the experiences which regularly accompany the essential integrative basis, partly to the fact that the *nova* are *nova* and can draw the attention and be compared and generally be the basis of new integrative processes, as if they were independent elements. From another point of view, however, this peculiarity is of great advantage; for it maintains the same freedom of mind for all stages of development. As integrative processes are originally spontaneous, the mind can accept their product without making special reference by attention or otherwise even to those experiences that form the essential basis of the integration. We can compare distances, tonal intervals, motions and melodies, without troubling to compare the orders and times that constitute them. We are immediately aware of the identity or difference of the mode itself in the various instances given. Thus the subjective efforts of the mind can be applied to any level or to any one of all the integrative processes which arise spontaneously upon any given occasion. This statement is absolutely thorough-going, as we have already noticed that every integrative process, no matter what its nature, must, to some extent at least, be spontaneous and automatic. Effort and attention may have to be applied indirectly to procure its appearance, as when we adjust our sense-organs, our body, our actions, our memories, our thoughts, in order to maintain a certain stream of experiences. But that stream of experience must, to some extent at least, flow spontaneously. The attention may then be applied to any point of it, usually its highest, in order to aid the spontaneous integration which is taking place at that point. The aid given may consist in rendering the integrative basis stabler, or in reducing the differences which present themselves to within the limits of spontaneous integration by means of special manipulation of the corresponding stimuli, or in repeating the series of integrating experiences so that the binding power of associations derived from simpler forms of integration may extend the integration in question over a longer stretch of time than that natural to the integration. What cannot be brought simultaneously within the compass of the mind, so as to integrate spontaneously there, may be taken in successive series and made to pass through the mind so



rapidly that it will then spontaneously reveal all its integrative secrets.

### C. CONCLUSION.

The first principle of integration is, by growing consent, almost agreed to already. In one form or another, sensory or motor, it is the only acceptable conclusion of the long-drawn-out discussion of the origin of local signs. They cannot be thought to originate out of the association or combination of anything that is not already local sign. What is derived is therefore not primitive local sign, but only the complications and modifications of local sign that arise under varying circumstances, on the basis of a correlation of the local signs of experiences of different systems, such as eyes, ears, vision and touch, touch and sound, vision and sound, etc. The same conclusion appears to be inevitable in the discussion of other important problems. The outcome of Jaensch's extensive investigation of depth is: "Die Tiefenwahrnehmung hängt aufs engste zusammen mit Wanderungen der optischen Aufmerksamkeit und den mit ihnen verknüpften Impulsen, also mit einer dem Gesichtssinn eigentümlichen Funktion. Hieraus erklärt sich, dass Tiefenwahrnehmung des Gesichtssinnes in keiner Weise mit Empfindungen und Vorstellungen, welche einem anderen Sinnesgebiet entstammen, identifiziert werden kann, sondern einem eben nur dem Gesichtssinn eigentümlichen Inhalt darstellt<sup>1</sup>." A similar remark may be quoted from a discussion of the various theories that have been given for the state of recognition. In criticising Rabier's theory, Katzaroff says: "Pourquoi ces divers sentiments qu'invoque Rabier, sentiment d'absence d'effort et de nécessité qui caractérise le souvenir par opposition à la fiction, sont-ils permutés dans la conscience en un sentiment de déjà vu, au lieu de rester ce qu'ils sont originairement<sup>2</sup>?" So also Titchener: "Wundt's theory is open to the objection urged against his theory of space. The blending of affective process with sensation means, elsewhere in the mental life, not time but feeling; and we cannot understand how, in this particular case, the new product should arise<sup>3</sup>." Every criticism of the insufficiency

<sup>1</sup> E. R. Jaensch, "Ueber die Wahrnehmung des Baumes," *Ztsch. f. Psychol. Erg.-bd.* 6, 1911, 357.

<sup>2</sup> D. Katzaroff, "Contribution à l'étude de la Recognition," *Arch. de Psychol.* 1911, xi. 15, cf. also p. 19 and elsewhere.

<sup>3</sup> E. B. Titchener, *Textbook of Psychology*, 1910, 347.

of mere association and the hopelessness of all attempts to come through with its aid alone are founded on this first principle of integration. Reid's answer to Hume's scepticism is the first step towards recovery from failure to do justice to the facts. The facts must be recognised. But this acceptance cannot now be framed so as to exclude further inquiry. For if some plausibility of derivation, some sort of resemblance, is what we desire, on finding it we necessarily accept the task of making an inductive study of these resemblances and of furnishing as adequate a theory of derivation as possible.

The second principle of integration is not by any means generally conceded. In fact it is usually implicitly denied. But whatever beliefs or prejudices may oppose it, it is the inevitable consequence of a systematization of the sensations and an essential part of any scientific psychology. It calls, of course, for the fullest experimental study of each mode of experience, both in respect of phenomenology and of function. The greater the disinterested devotion applied to its study, the more likely is it to be confirmed. For it promises the coincidence of broad rational demands with the facts, if only we treat the facts exhaustively enough. The psychology of the day presents many cases of difficulty and of opposition between reason and fact which call urgently for resolution.

The insight into the third principle is clouded by all sorts of philosophical generalities regarding continuity which do not attempt to define or to delimit precisely the mode of operation of the principle of continuity or to reconcile the demand for continuity with other legitimate demands. But the continuity and coherence are there. We do not need to create them; we have only to recognise them as they are, and to explain them. Recognising them for what they are cannot, however, mean attempting to maintain that experience brings no progress, no enrichment, nothing new, nothing more than was already within its compass. It is equally futile to barter the facts for a notion of self-development, or of the realisation of an end, as if that were a form of process in which all that is finally attained were already there from the lowliest form of consciousness, and so satisfied a craze for barren continuity. For purposive process in experience is itself undoubtedly a unique form of process, which therefore no more offers a standard for all other forms of integration than does any other unique process. If the continuity is there, we must just study it as we can and by inductive procedure extract from it what secrets it has to yield. Similarity is surely a kind of continuity. Whether it will suffice to cover the facts,

only detailed study can tell. But that it plays an important part in them, cannot be denied.

This third principle is indispensable in the formation of any theory that exceeds the bounds of sensationalism or its analogues. But it would be one-sided without the balance of the other two principles. Mere *nova* are inexplicable, whereas *nova* within a matrix of similarity offer the hope of an approximation towards completeness of theory. Even if distance were procured by a sort of sensory presentation of the orders intervening between those which bound it<sup>1</sup>, it could not be thought, as distance, to be a mere aggregate of orders, for it is more than that. It integrates these orders in a special way, which can only progressively be exhausted by knowledge.

This principle has another important aspect. It offers a basis for the separation of the objective mind and its processes from the subjective mind of effort, assent, attention, and the like. If we know that we have the objective mind before us at any point, we can hope to determine its scope progressively by following out the various steps of its integrative development. There is evidence that the processes of integration can be influenced in various ways more or less extensively by the attention, but it must be just as erroneous to suggest that they originate in the processes of attention<sup>2</sup>, as it would be to adopt the view that the mind involves only processes of integration of the kind found in the senses or in the cognitive states. If attention is involved in integration, it can only be supposed to support or to oppose the process of integration. It is not likely that the objective mind is a sort of image or parallel of the subjective mind of attention. Such a thing would not only be hardly intelligible, but it would refer or transfer all the problems of the objective mind to a shadowy world of subjective attention without any prospect of ultimate solution.

<sup>1</sup> Cf. Jaensch, *op. cit.* chap. 6.

<sup>2</sup> Cf. Jaensch, *op. cit.*, especially chap. 5.

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# THE ELEMENTS OF EXPERIENCE AND THEIR INTEGRATION: OR MODALISM.

BY

HENRY J. WATT.

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## THE ELEMENTS OF EXPERIENCE AND THEIR INTEGRATION: OR MODALISM.

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- § 12. *Conclusion. The classification of experiences. The gain for the experimental study of thought; and for genetic study.*



## § 1. INTRODUCTION.

A considerable time has now elapsed since psychology in its turn awoke to the new vigour of life that the experimental method brings to every science. Inquiry has been pushed into every part of the field and multitudes of new facts have been made known. These have been arranged as well as possible to show how one is dependent on the other or what is the joint effect of several. But nothing like a coherent body of knowledge has emerged therefrom. It is a common complaint that psychology is a medley of all sorts of curious and commonplace facts, which can hardly make a show of the coherence that is expected of a science. In many ways there seems to be no difference between it and physiology. Psychology stands at a certain disadvantage in this respect. For although the elements of its subject-matter are for it pure data, given without the possibility of further question, yet from the point of view of the biological sciences and perhaps philosophy in general, experience is an effect conditioned by physical and physiological circumstances. It is a curious fact that many prominent workers of the sister science of physiology have recently claimed the right to expel consciousness entirely from the scope of their subject-matter and from their list of conditions and results. However foolish and impossible this may be for the physiologist as inquirer, much may be said for it when the systematic ideal of causal explanation is the goal in view. For a mere knowledge of condition and effect is never quite satisfactory. It is this same spirit which has led some psychologists to banish any mention of body or brain from their treatises and to aim at a pure science of experience. Thus far, perhaps, the burden has lain more lightly on the physiologist, upon whom the stringently closed system of causes of the physical sciences acts with great compulsion. For the psychologist, however, it is no easy matter to set himself free. Only in respect of the intellectual and emotional parts of experience has it been attempted with any success, for we are, in any case, almost quite ignorant of any detailed connexion between these and physiological conditions. The psychology of the senses, on the other hand, cannot be loosed from physiology in any high-handed manner. By themselves, sensory experiences seem very erratic and peculiar. They seem too much the product of other influences and too independent of one another to form a closed field in themselves. And yet, as it stands, psychology cannot but be ashamed of its feeble command of the senses. Its knowledge of them is hardly more than

a mere bundle of clippings from physiology. And when the attempt is made to treat sensory experiences purely as such, the chapter on the attributes of sensation which results is so dry and barren, that it is condemned and omitted altogether by not a few writers.

A fresh attempt must be made to secure the independence of psychology. This will have little value unless the province of sensation is first attacked and freed from the domination of physiology. No general demand or principle will meet the case. It must be shown that physiology can make no positive contribution to the special work of a psychology of the senses and that the seemingly scattered and incoherent material of sensory experience is capable of self-complete and satisfactory systematisation. Only thus may independence properly be claimed. For we shall then have proved that psychology not only must, but can stand alone. It is, indeed, not to be forgotten that psychology and physiology are, in certain respects, closely attached to one another. Physiology provides a basis for experimental interference with experience which is invaluable to psychology, while psychology acts to some extent as a feeder to physiology. But however complete may be the parallelism between the two regions which general considerations lead us to expect, the promise of a comprehensive science of psychophysics seems to lie rather in the independent development of the two contributory sciences than in their narrow companionship.

It has been claimed sufficiently that introspective observation is the primary method of psychology. But every advance in the science sends us back to a more minute and observant pursuit of the method. If we are to convert our scattered sensory experiences into a coherent system, we must re-examine the whole field. We must note with all possible accuracy every variety of experience simple or complex. Every complex experience must be analysed into its simplest parts. But in doing so, we must not fail to observe whether the process of analysis destroys any feature of the complex experience, whose origin in the elements of our analysis we are unable to explain completely by synthesis of these elements. Just in this respect has psychology been in grave fault. We have been satisfied to know upon what conditions our complex experiences and their modifications rested, but we have not tried sufficiently to show how the elements of our experience combined to give our complex experiences. This must be attempted again and there can be no rest for our science till it is accomplished.

In those cases in which our experiences show variation in respect of any characteristic, the strict method of introspection seems to suffice.

Thus we know that some sensations have certain attributes in common, intensity, extensity and the like. But a number of cases are in dispute. Some sensations do not seem to share these attributes and some others seem to have peculiar ones of their own. Introspection is thus obviously insufficient to meet all cases. We must find another method whereby the properties of the elements of our experience can be determined. Like the chemist and the physicist we can find this only in a detailed study of the compounds into which these elements enter and of the manner in which they join to form complex experiences. We must know not only what are the elements of our experience and what are their essential properties or characteristics, as far as is possible by direct inspection of them, but we must also know the manner and method of their integration and other elements, so that by this knowledge we may be enabled to complete and perfect our knowledge of these elements.

But that is not all. Our knowledge of these elements and their properties must enable us to understand completely how, when a certain complex of elements is given, a certain complex experience results therefrom. No characteristic of the latter must remain unexplained. We must be able to give assent to a statement in the form of an equation, that this or that arrangement of the elements of experience, as we find them given, is wholly and completely identical with a complex experience. The notion of transcendence must finally be banished from any self-respecting science of psychology. In a word, we must be able to show the presence of causality in experience. That we have not been able to do so, has undoubtedly been due to the fact that the complexities of experience were taken too much for granted, that analysis was the prevalent method of study and that no attempt was made to show the connexion between the attributes of the elements and the integrative complexes of experiences and their modifications. No use was made of the attributes except that of unprofitable definition. It must speedily become clear that in psychology, as in the natural sciences, the problem of the elements and their properties is second to none other. Far from being dry and useless, the problem of the attributes must become a centre of the most vivid psychological interest. There can be no doubt of its difficulty. We must follow the example of the sister sciences of nature and converge the efforts of all pure mental science upon the problems of the constitution of experience and its fundamental laws.



## § 2. PRELIMINARY DEFINITION OF SENSATION.

A definition of sensation may be attempted by reference to its psychological peculiarities or to its psychophysical basis or to both of these. But as sensation is generally considered to form only a part of experience, it is hardly possible to begin study of the varieties of experience by enumeration of its purely psychological characteristics. For we do not yet know what these are and we must be able to point out sensation to one another ere we reach any sort of unanimity regarding its psychological definition. Besides, even although we may finally ascertain that certain psychological features are common to all sensations, there are difficulties in the way, which would become the greater, if we could not delimit the matter of sensation by other means. We must therefore turn to psychophysical means to fix our subject. We have sufficient security, if we find a means of pointing out to each other which of the whole mass of experiences we call sensation and intend to study. That means we find in the sense-organ and in its stimulation. We may accordingly define sensation as the simplest parts or elements of those experiences that are immediately and regularly dependent upon the stimulation of a sense-organ. Such a definition as this accords well with the practice of an experimental science. By means of a periodic recurrence of the stimulation and consequently of the sensation, it is easy to direct the attention of the observer towards the experience to be observed, while all possibility of confusion with other experiences which might be evoked along with sensation can be avoided by the observation of the parts of the total complex which recur regularly and without the mediation of any other experiences. Sensations are not attached to any other experiences as if they depended upon them. They form, at least in part, the groundwork or foundations of experience. As sensations are dependent upon the stimulation of sense-organs, they are clearly largely independent of such influences as attention and abstraction, so that we run less risk of error in starting our study with them. Having obtained a definite reference within the whole, we have provided ourselves with a means towards definite study of the other varieties of experience, as well as with a gauge for such variable influences as attention.

It is often said that there is no such thing as pure sensation to be found in experience and that sensation, therefore, exists only as a psychological abstraction. Without knowledge to seize hold of it and convert it at least into perception, it is declared, pure sensation would

be pure nothing, as unintelligible as is to the idealist an object independent of the mind which knows it. In the face of such an extreme view, any attempt to study sensation would be futile and objectless. But whilst we may admit that sensation never does in us occur as an object of study unless it evokes other mental processes than itself, whatever they may be, we may yet maintain that sensation is a real object of study. In the introspection of sensation, our observation is directed upon sensation as defined, in whatever setting it occurs. Sensation may often be observed to remain constant in character under differences in the introspective processes directed upon it. Besides, our definition of sensation does not call for the isolation of sensation in experience, but only for its isolated study.

While defining sensation in the first place by reference to the stimulation of a sense-organ, we do not forget that we are by no means sure of the position and nature of the sense-organs of all well-accepted sensations. Yet we are justified in regarding them as sensations, because we can verify their immediate and regular dependence on external stimulation. We know from obvious examples of sense-organs, how specific in quality and point of action the stimulation must be that is to affect a sense-organ and how dependent its success is on the integrity of afferent nerve-fibres.

The regulative simplicity of the definition refers in the first place to the experimental procedure implied therein. Simplicity of some kind will, of course, also be a characteristic of the psychological nature of the sensational element. But we cannot presume upon the ultimate psychological definition. We must just carry experimental analysis as far as we can, although we cannot hope to find therein any means of judging whether our analysis is complete. For a mere *ne plus ultra* cannot form a systematic criterion. Only in the psychological characteristics of sensations can we expect to find some such standard, the formulation of which will then constitute the psychological definition of sensation. Our only guide will therefore be the typical uniformity of sensation.

### § 3. THE INDEPENDENCE OF PSYCHOLOGICAL INVESTIGATION.

The chief classes of sensations are (1) those of cutaneous origin, touch or pressure, warmth, cold and pain; (2) those of taste, of which there are four chief varieties; those of (3) smell; (4) sound; (5) vision; of each of which there are an indefinite number of varieties, which

differ only slightly from one another and, except in the case of smell, can easily be arranged in order of resemblance; those of (6) articular, and (7) labyrinthine origin, each generally recognised to consist of two groups, sensations of position and sensations of movement; (8) those of muscular origin; and finally, (9) a crowd of less varied and obscure sensations.

All these varieties of sensation are said to differ from one another in quality. If the difference in quality is so radical that it is impossible to pass from the one sensation by gradual or imperceptible steps to the other, as *e.g.* that between tones and colours or between warmth and cold, it is sometimes called a difference in modality. It is generally considered to be of great systematic importance to determine all the possible varieties of quality and to arrange them as such and in relation to sense-organs and stimuli. Distinctions of quality are, of course, in all cases based primarily upon true introspective differences between sensations, but there is as yet no clear test of quality. Consequently a number of cases have been long disputed. Is pitch, for example, the quality of tonal sensations, and in what respect do the articular sensations from various joints differ? We must not forget that sensations differ from one another in other ways than quality and that, unless we are guided by some defensible criterion, we may mistake these other aspects for quality. This is most to be feared in those cases in which no variation in quality is really present. Any apparent variation is so readily ascribed to quality. The conclusions we shall reach later will show that the present classification by quality is not sufficiently critical and does not lead to useful systematic results.

In spite of the primacy of the introspective basis of quality, the relation to the sense-organ still exercises a fairly strong influence upon the distinction of qualities. It is generally admitted (1) that from one and the same sense-organ only one single quality or a group of closely allied qualities can be evoked. Conversely it is held (2) that each marked difference of quality or each difference of quality, no matter how slight, so long as it cannot be obtained from a mixture of other qualities, *e.g.* those of tonal pitch, implies a different sense-organ. It might therefore readily be supposed that the existence of different systems of sense-organs implied some qualitative difference between the corresponding sensations. Such is not the case. The above statements are still true even if the same quality of sensation may be evoked from different systems of sense-organs. Experimental research of recent years has, in fact, distinguished a superficial from a deep system for



touch and pain, and a protopathic from an epicritic system for touch, warmth and cold. It is, conversely, a familiar fact that similar series of visual sensations—all tones from white to black of fair intensity—may be evoked by the medium of different sense-organs, namely the rods and cones of the retina. There are no marked differences of quality between the sensations from these different systems of organs, but there is a tendency to interpret any obscure difference as qualitative (23, p. 36). In the case of labyrinthine sensation a qualitative distinction is usually made between sensations of position and sensations of movement and is justified by reference to the anatomical and functional independence of the two systems of sense-organs and the resulting independence of the two groups of sensations. The reasons generally given for the distinction of two groups of articular sensation (difference of threshold, different relation to galvanic interference) also imply a reference to sense-organs, although no duplicity of sense-organs has yet been established. We shall consider these arguments later in more detail. It is sufficient to point out here, that even if they were valid, they would not effect their purpose. The sensations in question, even though they were evoked from perfectly distinct systems of sense-organs, might still be of identical quality. Any difference in quality must be decided purely and solely upon the basis of an examination of the psychological characteristics of the sensations concerned.

The same conclusion is applicable to the sensitive areas of the eyes, ears and nostrils and to the multiplicity of individual sense-organs found in all senses. Each of these repetitions, of course, has its own special use. We may also expect them to be represented psychically, but we can only determine the nature of this psychical differentiation by psychical methods of examination and comparison.

Sense-organs may be reduplicated for other reasons than distinction of quality. A differentiation in respect of the intensity of stimulus may sometimes be necessary, as in the eye, where the rods respond efficiently to a stimulus which evokes no reaction from the cones, while the cones respond comfortably to stimuli which overwhelm the rods and necessitate their instant withdrawal from the full severity of the stimulation.

In the same way, we find in the sense of temperature a double form of apparatus, of which one, as judged by the sensations evoked from it, seems to respond vigorously and diffusely to all effective stimuli, while the other has a wide range of response and adaptation. There is no reason in this fact why the two series of sensations should not be closely related psychically.

We are therefore compelled to make our study of experience to a large extent independent of the physical and physiological study of the sense-organs. There is certainly a close relation of dependence between sensation and sense-organ, but we must beware of expecting a continuous parallelism between the arrangements of each. The very complexity of the more central arrangements of afferent fibres should warn us of this. We must therefore conclude that a reduplication of sense-organs of allied function may determine a variation of sensation by quality or by intensity, by extensity or by localisation or by some other aspect. What the variation in any case will be, we cannot tell by mere examination of the sense-organ. We must examine the experience itself. Not even in cases of doubt can we safely allow ourselves to be guided by consideration of local or functional separation in the sense-organ. We can expect to settle the classification of a disputed aspect of experience only by a direct study of it or by comparing it in form and function with other similar experiences. Our psychological interest lies only in the forms of variation of our experiences and in their functions as experiences.

Still more distant, therefore, must our interest be in those physiological processes inherent in the sense-organ which produce no new variation of sensational experience, *e.g.* adaptation, positive and negative after-effects, and contrast, while theories of the adequate or proximate stimulus to the sense-organ have no psychological significance at all. Our only interest in the sense-organ lies in the fact that it somehow makes a certain form of experience possible at a certain place and time.

#### § 4. THE TYPICAL CHARACTERISTICS OF SENSATION.

Typical characteristics are often distinguished and are commonly known as attributes, which are said to be inseparable from sensations and to be variable independently of one another. These attributes have hitherto been determined solely by mere direct inspection of the elementary sensations themselves and, as commonly accepted, include intensity, extensity, duration and perhaps order or localisation. Feeling-tone, which may be pleasant or unpleasant, has often been included amongst the attributes, but is now generally treated as an elementary kind of experience, qualitatively different from all sensations (14, p. 227). It is recognised that sensations often occur which are indifferent in respect of feeling-tone, which, in other words, are devoid of it. Besides, even when one and the same stimulus is used to evoke sensations, feeling-

tone varies so much in different people, that it might well be considered to be another kind of experience not directly and immediately dependent upon the stimulation of a sense-organ. Of course, a state cannot at one time occur with, and at another time without, one of its attributes, if this word attribute is to have its usual meaning. It is possible, however, that the separability of feeling-tone from sensation is only one of many indications that the various modifications of experience, of which the attributes form one group, are capable of much more freedom and complexity than has commonly been supposed.

(a) *Intensity.*

If attributes are inalienable accompaniments of sensation, we may expect to find them in the most elementary sensations—in those evoked by the stimulation of the simplest elements of sense-organs that can be functionally distinguished. Although it is by no means easy to determine the elements of even the comparatively simple sensory apparatus we find in the skin, yet it may safely be maintained that, as far as we know, a variation in the sensation from the simplest parts yet distinguished of accessible sense-organs like those of the skin, tongue and eye, is possible. This direction of variation or attribute is that of intensity, which is produced typically by an increase in the amount of the physical stimulus acting on the sense-organ. It is a peculiar fact that the lowest degree of intensity of similar sensations is not always so comparable as we should expect. The stimulation of a protopathic area of skin, for example, always evokes, when effective at all, a more vivid sensation than does the stimulation of normal parts. Weber's law, in fact, seems to hold only for the epicritic system; the variation of intensity found in the protopathic system is much more limited and rigid (23, pp. 50, 106 f.). The minimal degree of intensity produced by certain sense-organs cannot therefore possibly be considered to be the absolute psychical minimum for that sensation; hence it is illusory to say, as many do, that when the intensity of a sensation is reduced to zero, the sensation disappears, for we have no conceptual means of determining the degree to which any given minimal intensity approaches zero.

We also find that certain sensations vary in intensity very little or not at all. Such are the labyrinthine and the articular sensations of position in particular, and also some of the less frequent, miscellaneous sensations. Yet we could hardly maintain that these sensations offer to introspection no aspect of intensity. Only it is particularly hard



for introspection to seize hold of any aspect of experience that cannot be varied; for it is just by variation, especially in definite relation to changes in the evoking stimulus, that an experience offers itself best to introspection. We may therefore admit the presence of intensity in all sensations, except perhaps those of vision, where, though apparently present, it seems to certain psychologists to be merged in quality.

### *Extensity.*

Besides intensity there is no other obvious variation in the sensation dependent upon one and the same sense-organ. But another attribute can be made clear if we evoke the same quality from neighbouring sensitive elements of the same kind. Sensations of the same quality from neighbouring sense-organs stimulated simultaneously fuse with one another and give rise to a more extended sensation of that quality. In this form the aspect of extensity is easily observed, so that we can now detect its presence in the sensation from the elementary sense-organ, although it is practically devoid of all variability there. Differences in extensity can be traced between the correlated sensations of cutaneous origin. The sensations from skin-spots are undoubtedly extended; but that of warmth is certainly more extensive than that of cold and cold than that of touch or superficial pain. All protopathic seems to be much more extensive than epicritic sensation. Possibly extensity is now, for the most part, in that rigid undifferentiated condition in which we find intensity in the protopathic cutaneous, and in some other senses. It is hardly variable and therefore difficult to observe in labyrinthine and articular sensations of position and in the less frequent sensations. Some of these are, however, more or less massive or diffuse, so that we need not doubt its real presence in them. In smell it seems also to be latent. In sound it takes the form of voluminosity (*v. later*, p. 143).

In one group of cases only do we find a variation of extensity comparable to that of intensity. If *e.g.* the two eyes are directed in varying degrees of convergence upon two pictures which together give a clear binocular picture, it will be seen that the combined figure seems much smaller and nearer when the convergence is great and progressively larger when it is reduced. It is not possible to measure this variation of extensity, as we measure lines, by laying a graduated measure against it, for the measure itself changes in apparent size with the change of extensity of what we measure. We can only compare these variations

in extensity as we compare different intensities. This form of variation, like intensity, does not involve any change in the number or identity of the sensitive elements stimulated; for there is no change in the visual stimulus corresponding to the change of extensity. The changing stimulus lies elsewhere, probably in the kinaesthetic sensations connected with convergence and divergence.

Between extensity and intensity we find very often that there is less independent variability than the usual definition of attributes requires. An increase of extensity often leads to an increase in intensity and *vice versa*, so that in the judgment referring to the stimulus and based on sensations, an increase of the one in the sensation may lead to the judgment of the increase of the other in the stimulus. Explanations of this reciprocity of intensity and extensity suggest that neighbouring stimulations overlap to some extent and so become intenser, or that an intense stimulation radiates and so becomes more extensive. These explanations are, of course, physiological and not psychological. But the slight correlation of intensity and extensity thus given does not seem to call for any psychological explanation.

There are certain exceptions to the rule for extensity just given. The two ears and the two eyes are not two neighbouring sense-organs of the same kind; they are rather two sensitive areas or masses of sense-organs. When they are combined to certain special uses, other modifications of sensation than that of extensity appear. Extensity is not obviously given in sound; the same quality of sound does not appear in different extents, although tones of different pitch vary from one another in voluminosity. Probably the two nostrils act in ways analogous to that of the two eyes and ears, but our sense of smell is so degenerate and our knowledge of it so limited, that we may even suppose we make little use of the powers we have. Further consideration of these cases must be postponed.

### *Order.*

But even when two sensations are of the same quality, intensity and extensity, they can easily be distinguished from one another. Let, *e.g.*, two spots on either hand be isolated and stimulated in the same way. We can tell at once that they are two and from what parts they came, so to speak. We know "where they are." It is a familiar fact that the primacy of this local aspect of sensations has long been the subject of debate. And it may safely be said that the nativistic theory

is in so far correct, as some sort of inalienable aspect, responsible directly or indirectly for localisation, must be attributed at least to some sensations. Otherwise it is not evident how any sensations should ever come to be located. For differences of quality are not introspectively identical with those of localisation, or they would not be so easily distinguished from one another. If they are the same, there must be two kinds of quality—true quality and localisation-quality—which is the same thing as before. If it is meant that slight variations of quality combine to form localisation, it is not at all clear why just localisation and not some other form of experience should result from the combination. On the other hand, if only some sensations possess a local sign, it is not evident how the significance of that sign is to be transferred to any other sensation, even if the latter happens to accompany it regularly. It is not even evident that such a transference could take place without any mechanism, on the basis of mere contiguity in experience. For how could we expect experiences to attach themselves to one another, not to speak of interchanging characteristics, merely because they occur together? The only way in which they might do so is by mere mechanical association. We could not then expect to find that one would be for our experience attached and referred to the other as belonging to it, or that out of the connexion some new modification of experience should arise. For how could we claim to understand or to explain such occurrences? A science of psychology would here be faced with the unintelligible and irrational. The problem is merely a case of the general problem which is the object of our study: if a modification of experience is not common to the primary elements of it but arises with their combination, how does it so arise, and upon what aspect of these elements is it based? The formulation of such a problem calls for a vigorous protest against the admittance of irrational sequences in experience. The natural consequence of admitting such possibilities is the abandonment of every attempt to resolve them for psychological science. Salvation from such hopelessness could only be brought by some happy accident of experimental research. But if the mind is the instrument of rationality, we may at least expect it to be itself thoroughly amenable to rational, scientific treatment. And science cannot stop at the determination of mere dependencies; that would be a blind science, a science without the light of causal statement and conviction.

For the present, therefore, we shall accept naïvely the presence of a distinguishable aspect in sensations localised in different parts



of a sensitive area. There is no doubt that they can be distinguished primarily and in isolation, without the help of any sort of special association or inference. But it may be said at once, that this aspect of elementary sensations, though it undoubtedly distinguishes each element from others of the same kind, is capable of development along its own line, like any other attribute. There are complex, separable forms of localisation-consciousness, just as there are of quality, intensity and extensity. The success of our view will depend largely, if not wholly, upon what we can do with such a starting point. Its sufficiency and correctness may be questioned at present, if only for the reason that the single method of introspection, as we have already pointed out, is liable to error in dealing with the less variable attributes of sensory experience. But the results which our starting point leads to will ultimately justify it.

This third aspect of sensations we will call order. It is to some extent a form of individuation, by which sensations are differentiated, in the first place in relation to others of the same quality, but also ultimately to those of a different quality. But order is essentially an aspect of sensation, perfectly comparable to intensity and extensity. We might call it place in the mind, if it were not that place tends to imply that the mind has some real spatial extension, at certain points of which the sensations are to be found, whereas we have to remember that order is a place-aspect of sensations, which it qualifies, without any relation to real *locus*. Order is therefore the better name, as it involves only the idea of distinction, relatively to others of the same quality, intensity and extensity, by means of an aspect of arrangement inherent like these attributes in the sensation itself. The order of every sensation is fixed relatively to all others present, but does not depend upon the number and kind of these. Two sensations are not as such of neighbouring order because they are alone together in consciousness. They may still be of very different order. This attribute of order is much more important than any of the others for the development of experience and especially of the higher reaches thereof. It is the basis of all kinds of localisation and of many other complex modifications of experience.

Order is present in the form of localisation relatively to one another in all sensations, except those of sound and smell. In the case of articular and labyrinthine sensation, it seems much more advisable to treat their differences as differences of order rather than of quality. How should we otherwise be able to treat them as a system of interrelated positions? Sounds are, of course, localised in space, but this localisation is known

to be not a primary peculiarity of these sensations, but an integration based on the simultaneous use of the two ears in ordinary cases and perhaps also often on differences of timbre or harmonics. Sounds are not localised at a certain point of a sensitive area; indeed it is improbable that each ear contains neighbouring sense-organs of the same kind. Sounds, however, assume an order relatively to one another in the form of pitch. In smell we are unable to suggest any primitive aspect of order. Smells are localised by a secondary indirect process, similar to that of sound and most usually by variation of intensity, consequent upon turning or approaching the nose towards the source of smell. Have we now so little versatility in smell that we cannot pick up the lines of their order? It is impossible to say. Our very ignorance regarding smell constitutes by itself one of the most difficult of psychological problems. The two ears, the two nostrils and sometimes the two eyes do not afford us sensations which differ in order, so that sensations of the same quality from each of these pairs of parts do not give rise to the same modifications of sensory experience, as do sensations of different order from a single sensitive area.

#### *Other aspects.*

To the above three attributes a fourth—order in time, duration, or protensity (24)—is sometimes added. There are many good reasons why such an aspect should be expected. But many difficulties lie in the way of its study. It is for one thing very hard to decide the simple introspective problem whether the order in time of an experience is a true attributive aspect, or is merely position in real time, identical with the temporal succession of events in the material world. Two sensations may be of the same quality, intensity, extensity and order, and yet be distinguishable. But is this distinction not a purely conceptual one? On the other hand it may rightly be asked whether any such conceptual distinction could be made, unless it had first a basis in sensational experience itself. The answer to this very important question is one which will be decided largely by the results of a study of those attributes which are clearer, as well as of the complex modifications of experience which result from them. For the present, we may without inconsistency decide in favour of an attribute of time-order or protensity of some kind, without attempting to give it a precise characterisation. Indeed, further study may give us reason to look for and find still other attributes of sensation than those enumerated. The

problems raised in this paper can only be worked out in detail for a few cases, but it will be evident that they are very general. The study of the one will act and react upon that of the other.

(b) *Difficulties.*

Study of the attributes of sensation allows us to institute a comparison between such senses as those of cutaneous origin and that of hearing, which gives results of great advantage to the further study of the modifications of sensation. This comparison is based in the first instance upon introspective evidence, and finds its further justification in the psychological results which emerge from it.

Cutaneous sensations are varied by the attributes of intensity, extensity and order, but they show little variation in quality. Hearing, on the other hand, has a great variety of distinguishable "qualities," which are undoubtedly capable of variation in intensity. But these qualities do not seem to be extended after the manner of cutaneous sensation; and in so far as only simple forms, poor in harmonics, are presented to one ear, they seem to be devoid of any sort of localisation or order. Such is the result of what might claim to be a simple unreflecting comparison of cutaneous and auditory sensation. Sound seems to diverge more from the probable "type" than any other kind of sensation.

Closer consideration, however, leads to a very different conclusion. Instead of having given a purely unreflective, unbiassed judgment, we may possibly have been influenced very strongly by a comparison of results. Have we not really been comparing the outcome of integration in cutaneous with that in auditory sensation? Have we not, in fact, argued that, if sound had extensity, it ought to give us the sort of spatial extensiveness that we find in touch and vision, and that, if the elements of sound from one ear had order, they should arrange themselves over our skin or in space around us, unaided by the other ear or by differences in timbre? That we have indeed done so we shall best and easily find out, if we ask the question, whether the aspects inherent in sound, both in their primitive and in their complex forms, are closely comparable with those of touch and vision. If they are really comparable, we must, of course, look for some reason for the differences between touch and vision which influenced us in the first instance. If we can find it, we shall have clear proof that we were influenced in our argument by a principle we failed explicitly to state or to adopt: that like attributes should lead to like integrative results.



*Sound.*

Are the attributes of sound and touch, then, exactly comparable? There can be little doubt but they are. The manifold "qualities" of hearing are capable of the most definite arrangement in one continuous series of tones from the lowest to the highest pitch. No other "qualities" fall of themselves into so precise and unmistakable an arrangement. What evidence has introspection to offer against the classification of these differences as differences of order? On the contrary, introspection can justify such a treatment now, as it did long ago. "Till the time of Aristotle tonal qualities were considered essentially as a *ποσόν*, not as a *ποιόν*" (25, vol. I. p. 136, note). In fact, in spite of definite rejection of this view, it is hard for the adherents of the qualitative view of pitch to suppress the tendency to treat it as order. The qualitative order of tones is said to be "analogous" to the spatial (*ibid.* vol. II. p. 55). Mach, indeed, traces an analogy between the fixation of spatial points and the fixation of tones (18, pp. 182 ff.). Let us, therefore, frankly treat pitch as order and see what the result will be.

Tones are generally recognised to vary in voluminosity progressively, the deepest having the greatest, the highest the least volume. Pitch and voluminosity cannot be identified with one another, for we are able to discriminate differences of pitch much more keenly than differences of voluminosity. Several tones of the same or of different pitch sounded together do not give an increase in voluminosity, as we should expect. They fuse in extensity as little as do the extensivities of tactual sensations from the two hands. But for the former there may be forthcoming as good an explanation as we can give for the latter. The facts, therefore, should not prevent us calling voluminosity the extensive aspect of tone, if we are justified by introspection in doing so.

If we recognise, then, that, just as all tactual sensations have the touch-quality in common, so all tones share the same sound-quality, we shall have our full complement of attributes: quality as such or mere sound, order or pitch-place, and extensity or voluminosity. Beyond these three aspects, tone does not seem to have any other characteristics than intensity and those that are the result of the integration of different tones. Even timbre is shown to consist of, or to be actually analysable into, separate tones, each provided with the aspects we have enumerated.

The peculiarity of tone is that of these attributes two—pitch (order) and voluminosity (extensity)—are mutually dependent variables. It is impossible to run through the variations of pitch without at the same

time varying voluminosity and *vice versa*. As tone is elementary sensation and as pitch and voluminosity are its primitive attributes, it is impossible to look for an explanation of this interdependence of attributes elsewhere than in a physiological theory of the sense-organ. Such an explanation, if it is not actually there already, may be said to be in sight at the present time. But it cannot concern us here. We can do no more than acknowledge the introspective fact of interdependence of pitch and voluminosity.

For this very reason it is evident that extensity in tone cannot, as in the other senses, be a variable dependent upon the occurrence of many tone-sensations of neighbouring or different order. For as each pitch or order has a different voluminosity inseparably attached, though easily distinguishable from it, the aspect of pitch will always suffice to segregate its fellow voluminosity from others, even when there might be a tendency for them to fuse in some manner, and *vice versa*. Many tones together, therefore, will not fuse, as sensations of the other senses do, in any way except intensity<sup>1</sup>; and even that will occur, of course, only when the tones are the same in quality in its threefold aspect, *i.e.* in pitch. In other words, tones of different pitch sounded together will always be distinguishable, even if they are not always distinguished from one another. It need hardly be added that sounds fuse together as mere sounds apart from all aspects or attributes.

Many high tones of nearly the same pitch, even when they are consonant, would not therefore give a voluminosity equal to that of a tone of great depth. The voluminosity of all together may very well differ from that of any of them alone, in a way peculiar to such combination, but it will never approach towards that of a deep tone. For this and other reasons it will often occur that many tones together are not distinguished from one another, but they can always be distinguished as soon as the attention is directed upon their order or pitch, their voluminosity, or the integration of these. For if homologous aspects of two tones are distinguishable from one another in isolation, they cannot be completely fused with one another when they occur simultaneously.

We have thus brought the introspective nature of auditory sensation into line with that of all the others except the visual and olfactory. And we have explained the first apparent discrepancy between tone

<sup>1</sup> Or timbre, which we may neglect for the moment, as it is obviously not a characteristic of the elementary sound. Timbre, as an integrated character of tone-complexes, forms an interesting problem for psychological treatment.

and the general type of sensation. But it is generally recognised to be very difficult to decide whether noise is a unique quality of sound or not. Our revision of sound will here also give us a definite point from which to proceed. A noise may be said to be a simple sound whose pitch is not yet audible, because it has lasted less than the time of two vibrations, or a complex sound of many pitches which make each other indistinguishable to the unaided attention. We have good reason to let this definition pass, as we know similarly from the sense of pressure that it takes less time to be aware that we have been touched than to be aware where we have been touched. This is Külpe's law, that "general denominations are more easily reproduced than special" (14, p. 172).

If our analysis is so far correct, we shall expect to find it justified by the nature of the modifications which result from the further integrations of tone-sensations. These integrations should be parallel in mechanism and effect to the integrations of similar attributes in touch and vision. On the other hand, the treatment of pitch as quality defers indefinitely all hope of explaining the facts regarding melody, interval and tonality, besides those of discrimination already mentioned. There is also evidence of a genetic nature to show that the sense-organs of hearing have in all probability developed out of those of a sense with the full number of attributes, viz. pressure. But our argument can hardly lead us to suppose that sensations of hearing have actually developed out of those of pressure. For the skin sensations, whose sense-organs might also be connected genetically with those of pressure, all show differences of quality without any obvious loss or integration of attributes. If any theory of psychical development is suggested by the analysis of the attributes of hearing and their identification with those of the other groups of sensations, it must be one which traces all varieties of sensation back to a common origin or at least to a common type.

### *Vision.*

We have already noticed that visual sensations are characterised by the attributes of order and extensity. Certain observers, however, hold that they are devoid of all intensity. The intensity which is apparently present, it is urged, is really a form of quality<sup>1</sup>. It is clearly impossible to settle this question on its introspective basis alone.

<sup>1</sup> For references, v. 28, pp. 21, 324.



We must look for some other ground of argument. It is a further peculiarity of vision that it offers the vast range of progressively different qualities indicated in the colour-body. All other groups of sensations than those of vision, hearing and smell, occur only in one, or perhaps sometimes a few discrete qualities and do not seem to lack any of the usual attributes except perhaps in virtue of their obscurity to introspection. We should hardly venture to urge a plea of obscurity to excuse the apparent absence of certain attributes in vision and hearing, although the plea might hold for smell. It is of interest to recall that the great variety of sound-qualities can be explained by the variation and integration of extensity concomitantly with that of order. Only one form of quality similar to the unique qualities of pain, pressure and most other sensations except taste need be postulated. This integration, further, has to be accounted for by reason of physiological determination not of any special psychological integration of pitch and extensity; for, though correlated differently in sound, these are attached to one another in the same way in all sensations. In pitch we have still obviously a difference of psychical order, now inseparably attached to quality. There seems to be a great variety of sound qualities, though there is clearly only one.

We might therefore surmise that the typical form of elementary sensory experience is such that, when a difference of quality occurs, it is a radical difference and that these elements of experience could not be expected to fall into different classes of very similar sensations, such as those we find grouped together as visual, auditory and olfactory. We should rather expect discrete forms, such as touch, pain and more especially cold and warmth, which, though they are both concerned physically with temperature, have nothing in common as sensations. Primitively we should have one sound experience, one or at most a few unconnected and dissimilar visual experiences and a few for smell<sup>1</sup>. For a sensation which has a number of variable aspects must have some unchangeable aspect. Why should it otherwise be called one? If there are any primitive visual experiences, it is certainly difficult to locate them purely psychologically amidst the flux of qualities. The great variety of visual experiences, therefore, calls for some explanation. This explanation must, however, be left for the

<sup>1</sup> It is interesting to notice that it is in these complex senses only that we find most evidence of physiological integration and in particular the processes of compensation and rivalry. This would suggest that even the four qualities of taste are too many for one sense and that without integration only a single quality is found.

future to bring. The need for a psychological theory of vision is great, for we have none as yet. Our theories of vision run out into pure physics or physiology and leave all purely psychological problems entirely alone.

### *Smell.*

Of all the sensations, those of smell offer the greatest resistance to any form of investigation. The mere difficulty of manipulating the smell stimulus is overwhelming. We have practically no kind of a theory of smell at all, physical or physiological. The slight clues given by partial congenital anosmia and olfactory fatigue have led to no tangible results. In psychological theory, where we might reasonably expect to be less hindered, we are quite as badly situated. For we have only a tentative and imperfect classification of smell qualities at the best. These seem to be of indefinite number and devoid of all extensity or order. This ignorance constitutes of itself an important puzzle. For if the rich and progressively differentiated varieties of our experience, including those of the senses, are derived from simpler, more abruptly differentiated elements, as we must suppose, it is difficult to understand how this process of integration can be completely hidden as it is in vision and smell<sup>1</sup>. For vision we might suppose that we just do not yet see what is there to be seen. For it is a postulate of our whole treatment that the elements of experience are not lost to view when they integrate to form some new modification of experience, but that they may be seen in the integrative modification once we can read this rightly. But we have to remember, on the other hand, that these elementary integrations are always physiologically conditioned, although their form must follow psychological lines; special physiological conditions may make the process of integration very complex indeed, especially in highly developed senses, such as those of smell and vision. Thus we may expect that the unravelling of such difficulties may come rather by means of physiological experiment, than by unaided psychological analysis. Whatever happens, there can be no doubt that the psychological result must consist of a reduction of the complex progressively variable qualities of vision and smell, characterised by peculiar attributes and wanting in some of the usual forms, to a few simple abruptly different elementary sensations, characterised by the typical attributes.

<sup>1</sup> But cp. feeling, later, p. 193.

*(c) Conclusion.*

The typical characteristics or attributes of sensation may therefore be put down as (1) quality, in virtue of which sensations fall into separate species, abruptly differentiated from one another thereby, (2) order, which constitutes the individuality of single sensations of the same quality and gives them a definite place in the total experience of any one moment, (3) intensity, by which a variation of each individual sensation is made possible, and (4) extensity, by virtue of which each individual sensation is capable of continuous fusion with others of the same quality, whatever be their intensity, so long as they are of neighbouring order. It is a peculiarity of extensity that it is not bounded by precise limits; and for this and other reasons it cannot be argued that the distinction of elements of experience is fallacious and destructive on the ground that we should never be able to understand how such discrete elements fuse and combine with one another. No real psychical limits are presupposed by the distinction of elements of experience and their typical characteristics. In spite of the difficulties of vision and smell, so many different kinds of sensation do actually show all these characteristics, that we may expect every elementary sensation to be characterised by them. We have the more reason to assume this for sensations of certain kinds which, as we have remarked, show little or no actual variation by way of these attributes, since we have good cause to believe that the occurrence of many of the possible variations of cutaneous sensations, such as those of temperature, is dependent upon the range of function of the sense-organs which subserve them. There can be no doubt that the most highly developed senses are those of sound, vision and smell. Sound, which is still clearly in course of development, as the peculiarly rapid advancement of the musical art indicates, we have already reduced to the type. Vision is even more complex, but it still stands close to the type, except for the alleged absence of the attribute of intensity. As the linear series of progressively different tone-qualities is explained by integration of a single unchanging quality with other attributes of the type, so we may hope to explain the tridimensional variation of visual qualities by a similar process of integration. The vast and probably multi-dimensional variation of smell qualities would suggest perhaps a still more elaborate process of integration. One attribute of smell which might account for some part of this, at least, is missing, viz. order. It is uncertain whether



extensity is also missing there or is only difficult to observe, because it is not integrated to varying a real extensity, as in touch and vision.

It is important to emphasise that the problem of the elements of sensory experience and their typical characteristics forms the central and essential problem of any psychology of sensory, if not of all experience whatsoever. For, as we shall endeavour to show in the following pages, it is by means of the fusion of variations in these attributes, that elementary sensory data are linked and integrated into complex experiences, which contain these differences in them subsequently in the form of new modifications of sensory experience attached to the whole unity of integrated data. An architectonic of experience is as unthinkable without the attributes, as is an architectonic of matter without the physical and chemical properties peculiar to its elementary constituents. Far from being the outcome of meaningless psychological abstraction, the problem of the attributes is vital to the existence of any pure science of psychology; and its progress is dependent not only on the means of observation peculiar to psychical subject-matter—introspection, but it is assisted enormously by a study of the forms of compounds which experience shows. It must be our next task to analyse as many as possible of these compound experiences, and to ratify, extend or correct our knowledge of the attributes of elementary sensation by means of the knowledge of the mechanism of combination we thus gather. A means or basis of combination is always necessary; for we must remember that experiences, whether elementary or compound, cannot be expected to arrange themselves by any means external to the mind or not operative in the mind. They must arrange themselves entirely by themselves, purely in virtue of their inherent psychical powers. We expect, of course, some sort of parallelism between the psychical and the physiological, so that we may trace the dependence of the one upon the other. But we have, as yet, no hope of explaining the characteristics of the former in terms of those of the latter. It is still more vain to suggest that physiological arrangements explain a form of psychical arrangement which is not grounded in characteristics inherent in the psychical elements themselves. The physiological arrangement, doubtless, determines the latter and is, of course, a valuable item of knowledge. But an explanation of psychical arrangement must be full and satisfactory and must carry conviction in itself. Experiences hardly ever come singly and successively, or in pairs and simultaneously, so that they might be connected or arranged by mere isolation; they come always in crowds. Why, then, should one of them

link with another and not with a third, if not by virtue of the intrinsic affinities of their characteristics? No external power of body or of will could rule them. This reflection is often ignored by those whose interest in the study of experience is partly or wholly physiological, and by all who take the orderliness of experience, as it stands, for granted.

And it is just in the attributes that this means or basis of combination of the elements of experience is to be found. What could be more likely? Where else should we look for any means of combination? The states which result from the combination of the elements of experience show an introspective character which stamps them at once as elaborations or secondary forms of the primary modifications of experience, the attributes. Nothing could be more plausible than the theory that all secondary modifications are derived from the primary attributes by the integration of differences of the elements of experience given in respect of one of these attributes. We have every reason to maintain this for all secondary modifications until we meet with some pure datum of experience other than sensory. Integration must result in a modification of the integrated attribute. We cannot expect to find a modification of extensity resulting from differences in the attribute of order of the sensory elements given, or a modification of order consequent upon differences of intensity. For we should not be able to give final assent to any such equation and should thereby fall short of our ideal explanation. By experimental investigation we may exhaust all the discoverable conditions which affect an event, but experimental exploration can never be enough. Our knowledge can never be complete, till we can supply a convincing causal identification which contains evidence in itself that it is complete. We must be able to show that, in respect of some one aspect, conditions and event are identical. This is clearly impossible, if on the one side stands intensity, on the other order, no matter how clearly we may have shown a correlation between the two sides. To uphold this position, however justifiable it be, calls for some courage. For we find in the psychology of the day quite a number of these irrational sequences. Only one need be mentioned; it is commonly held that our localisation of sounds is dependent upon the difference in intensity of a sound as it reaches the two ears. As it stands this may be true. But that psychically realised differences of intensity of sound turn into or produce of themselves a localisation of that sound is a proposition no one can assent to. Either the facts, as stated, are wrong, which does not seem to be the case, so careful and repeated has been the

experimentation on this question; or differences of intensity in physical sound evoke some hitherto undetected attribute of sensation, which, along with the sounds given, suffices by integration to yield psychically localisation of sound. If we can discover this integrating attribute of sensation, we must then be able to assent to the identification and we shall be justified in considering our statement as final, unless experimental exploration shows us that we have omitted one or more stages. In any case, the final statement must be convincing as such. Nothing less than this can be our ideal, if we are ever to have a causal science of pure experience.

### § 5. THE MEASUREMENT OF EXPERIENCE.

It is a familiar fact that the attempt has been made to measure the variability of the simple sensation. And as intensity is the only attribute of the elementary sensation from the unit sense-organ that is capable of variation, it is natural that the effort to measure should have been concentrated upon this attribute rather than upon the others. Yet one might have thought that the idea of measurement was more applicable to the attribute of extensity; for the simple sensation provides a natural unit of extensity, whose multiplication seems to lead to an increase of that attribute. But it will be remembered that this increase of extensity, which can be measured by the conformity of a unit-standard with parts of the amount measured, is not at all a variation of extensity comparable to the psychical variation of intensity. The extensity of one and the same elementary sensation is never variable, and in sensations of any one class it is usually found in a rather rigid, undifferentiated state; it seems to find true variation only in vision and sound. In vision, its variation is dependent upon change in convergence; in sound, we find it attached to pitch in the form of voluminosity, which is variable, but does not grow by the accretion of sounds. In regard to order, it was hardly to be expected that the attempt to measure should be extended thereto, for no elementary sensation differs by itself in order and each elementary sensation has a different form of order from every other. Only in sound has order, in the form of pitch, been made the object of measurement and there it is notable that the usual results are not obtained.

But even though intensity seemed to invite a quantitative study, it is obviously impossible to apply the quantitative concept to that attribute. For there is as little hint of any distinguishable unit in



a given intensity, as there is in a given order or in extensity in the strict sense. Nor does the fixation of an arbitrary scale of just perceptible differences of intensities attributive to different sensations lead to any other reality underlying or conditioning intensity than those of a physical or physiological nature, which are already sufficiently measurable. More decisive than all else is the fact that we cannot manipulate our arbitrary unit, however chosen, so as to add it to or take it away from a given sensation.

It is hardly possible to bring further argument to bear against the possibility of measurement. We cannot hope to make one aspect of experience the basis of standardisation of any other. We should expect with as much reason to succeed in applying the notion of sensational intensity or extensity to the quantitative concept as to succeed in applying the terms of conceptual quantity to sensational intensity. Just as great is the world of difference between the order-differences of sensation and the conceptual orders of a mathematical or of any other system. These and indeed any other psychical characteristics are utterly incomparable and incommensurate. We can, therefore, only demand that the lines of variation of experience be carefully observed and compared. It will then become evident that experience varies along certain lines in ways peculiar to itself. A multiplication of units would not constitute variation at all. Nor is anything to be gained by the supposition that these variations are really quantitative; for the actual variations in any modification of experience serve us well enough to indicate the physical stimuli which evoke them and to enter as such indications into the work of the mind. When we find, as we do, that these stimuli can be treated and manipulated as consisting of unit-amounts, the variations of our experience will serve to indicate their presence and action and will stand conceptually as indices of quantities. It is not our concern, nor is it possible to show at this point how this takes place.

The question whether experiences may differ from one another without being recognised as different, does not arise here. For a slight difference by way of variation may just as well pass unrecognised as a slight difference by way of quantity. There is also a great difference between the mere presence of differences and the distinction of differences. The integration of differences and the process of distinction of differences have each their special conditions, which are not necessarily the same (cp. below, p. 176). Outside of these limits we cannot expect differences to lead either to any form of integration or to their own

distinction. Indirect proof of their presence therefore creates no problem. Turn the matter round as we may, we never do more than recognise differences by variations of certain modifications of experience directly or indirectly, as far as is possible.

In this connexion it is important to notice that, besides the primitive attributes already treated, we find in sensory experience a number of secondary modifications, each of which has its own peculiar manner of variation. Examples of these modifications are motion with its variation by speed, distance with its much less marked variation by extent of distance, and depth. Motion and distance we shall study in some detail immediately. These modifications have not usually been held clearly before the attention in the treatment of the problem of measurement, although quantitative experiments have been carried out upon them. The reason for this neglect is that they have not been treated properly as modifications of experience. We may say generally, however, that the problem of mental measurement and any formula such as Weber's law are applicable only to variable modifications of the same nature as that of intensity. We may also with much safety assume that where a threshold and a just perceptible difference are determined, we are there dealing with one of these variable modifications of experience. So many quantitative determinations have been made of distance in the form of discrimination of points that it is surprising that explicit reference is not always made to the fact that distance is a modification of the same peculiar kind as intensity, with a line of variation of its own. For that and other reasons the work on the discrimination of points looks awkward and out of place in any systematic treatment of psychology, unless it is recognised for what it is: the investigation of the discriminability of orders (primary attribute) and of distances (secondary modification, *v.* § 7).

The attempts that have been made to measure sensation have sometimes been characterised as the determination of sense-distances or of distances between the different points of variation of any modification of sensational experience, as fixed with reference to the evoking stimuli (27). We may, for example, judge that one degree of intensity is as far above another as the latter is below a third, and the like. If there is any such distance which may be presumed to be objectively fixed and constant, *e.g.* the just noticeable difference, it may be adopted as the basis of measurement. Our measurement with this unit will be as real as is the measurement of height, time and weight, for what is measured is in these cases never magnitude, but merely the distance between

limiting points ("magnitudes"). "The prototype of all measurement is linear measurement in space" (27, pt. 1, p. xx).

We must be careful to see that we know how much is involved in this statement. Spatial points have certainly no magnitude. But they are equally devoid of any inherent qualitative character. For conceptual science they can be fixed only by their relations of distance to some fixed point. But neither this fixed point nor the unit of distance has any inherent qualitative fixation in science. Hence the necessity for science of finding some natural unit of distance which is independent of the immanent qualifications of our experience; hence also the impossibility of finding a naturally fixed position. For natural distances, *e.g.* wave-lengths, are repeated and therefore lend themselves to conceptual treatment, in so far as they may be presumed to remain constant in repetition, in spite of the inconstancy of their bounding positions. It is therefore enough, if these can be fixed in attention for a short time. But position cannot in turn find its ultimate fixation by reference to distance. Being in itself nothing, it can be fixed only by reference to the inherent specifications of experiences.

It is the peculiarity of experience that each part of it contains its own qualitative characteristics apart from all relation to other parts. These characteristics we have found to be quality, intensity, extensity, order and, perhaps, protensity. Even the point or "spot" of sensation is qualitatively fixated in a way that is independent of all real positions and of time. These immanent characteristics cannot be taken over by science into its conceptual schemes, so that they must be converted into conceptual indices, based upon processes as independent of experience and its intentions as possible. But it would be a mistake to suppose that science is interested only in the fixation of points by conceptual distance-references. It wishes, wherever possible, to state the actual composition of these "points" themselves. This weight, it says, is 1 cwt., or one hundred weights. When it says a hill is 1000 feet high, it does not mean that the highest point of it is itself 1000 feet. It has, in each case, to consider its own intention and the license of the real facts. The inherent indications of experience cannot be treated conceptually. Any success in doing so would destroy them utterly. But they could be arranged conceptually by reference to one another. One must, however, remember that this reference (sense-distance), as a scientific instrument, must itself be purely conceptual. It has for its unit a process whose constancy is presumed, but whose nature is hardly understood—distinction of difference. It cannot properly be compared



with the distance between points in tactual or visual space or purely sensory distance. Distance is as direct as feeling and as *anschaulich* as intensity or depth. What the distinction of differences is, we do not yet know, but it can, at least, never be identified with sensory distance. For sensory distance is given only by sensations which differ in respect of order. It does not result from differences in respect of intensity, extensity or quality.

In a word, the whole work of measurement from the purely psychological side ends just where it began, in the determination of relations to effective stimuli, to favourable and unfavourable circumstances, of the just noticeable presence or difference of experiences or of their modifications. As experiences and more especially their modifications can be made to vary regularly in most cases, although some are practically unvaried, just noticeable differences between these variations are thereby implicitly arranged. That the stimuli corresponding to these just noticeable differences stand to one another in average cases approximately in a certain relation, is an important fact, but it tells us nothing about the experiences that was not already revealed by the changes of these experiences themselves. As the distinction between two variations of any modification of sensational experience is not itself a modification of sensational experience or a variation of such a modification, but a different, later and probably highly complex mental process, it follows that the determination of just noticeable differences will be subject to a number of influences of a purely psychological nature, which we cannot at the present moment understand or systematise. They may therefore be put aside as belonging to another part of our study, although they may serve there as an important basis of research. It will also be clear that we may pass by all detailed questions regarding the stimulus-values of thresholds and just noticeable differences. The value of these, as evidence of the existence of a relation of dependence between one mental state or modification and another, has probably been very much overestimated. For these values depend, as we have already noticed, very largely upon physiological conditions in the sense-organs and do not seem to be due to purely psychical restrictions. We may therefore expect them to fluctuate so much from type to type and from case to case, that their values for psychological theory can only be the slightest. We are therefore free to proceed with our study of the varieties of experience.

As no true measurement of experience is possible, we cannot expect the mind's evolution to be based upon its measurement of itself, or to show

quantitative laws. Nothing is given but a number of experiences qualified by certain variable or unvariable aspects. The mind's evolution must therefore rest upon these differences. We must expect to find that the widest use is made of these differences. Far from being a hindrance to unification and progress, they are just what makes these things possible.

## § 6. SECONDARY MODIFICATIONS OF SENSATION.

It need hardly be said that all secondary modifications of sensation must be observable directly; their presence may not be inferred. Changes of a peculiar indescribable kind, evident only after direct experience, supervene under certain circumstances, and though seeming to add something to the complex of sensations to which they are attached, nevertheless do not so radically change their sensational foundations that the identity of these before and after their appearance is ever in doubt. As modifications of sensation, they are distinguished from other modifications of experience in that they are dependent upon the stimulation of sense-organs for their first occurrence at least, and that, in their full variety and distinction, they attach only to sensations. They can be distinguished from the attributes of sensation by the fact that the latter are hardly separable from sensation at all, as far as we know; whereas secondary modifications never accompany the single sensation derived from a single sensitive element. On the contrary, they are always evoked by the action of stimuli on two or more sensitive elements, unless successive stimulations of one sensitive element suffice. From the psychological side they presuppose the simultaneous or successive conjunction of two or more sensations. While these necessary conditions are always complex, they are not always of the same nature. Sometimes the stimuli or the sensations refer to one and the same sense, sometimes to different senses, while the modification which results forms an extension of that attribute whose differences are integrated. The study of the secondary modifications of sensations will therefore be rather complex, and will in any case involve consideration both of their introspective nature and of their sufficient conditions, in so far as these are of a purely psychological nature. It will be necessary in each case to find for each secondary modification of sensory experience and its variations not only an unambiguous complex of sensory data, but to show how certain variable aspects of these can be identified with the modification which results from them.

For the present, however, we shall study only two of these modifications—motion and distance—and the simplest and most primitive forms of these.

### I. *Motion.*

When we cast around for further differences in sensations than those already mentioned, we cannot fail to have our attention drawn early to one of the simplest and biologically most important of all further warnings from the environment of an organism, viz. motion. In its generic form, *motion is obtained when successive sensations from neighbouring, or, within certain limits, separate sense-organs of the same kind, differing at least in respect of the attribute of order, fuse with one another.* We shall refer later to the limits of difference of order within which the integration of motion can occur. For the present we shall neglect them and consider only the case of continuous motion produced by a moving stimulus. Motion is found developed upon every group of sensations which show distinct variations from one another in order, viz. the cutaneous sensations, especially touch, articular sensations of position, visual sensations and also auditory sensations, where it is known as melody.

*Cutaneous.* On the skin it is found that every nerve-ending and every touch-spot can be distinguished from every other, with the exception, perhaps, of those that lie too close together to allow of isolated stimulation. If this result is to be obtained, certain precautions must be taken. The stimulation must be confined to the two touch-spots to be examined, a sufficient pressure must be used, as nearly equal in the two cases as possible, and a certain interval must be allowed to elapse between the two stimulations. If two points are stimulated in this way, we have the impression that the stimulus has moved on the skin (10, pp. 721 f.). Motion is thus found in its simplest and clearest form in passive cutaneous touch. As a secondary modification it rests in this case solely upon the difference in order of the sensations from two neighbouring pressure-points.

*Articular.* Motion is developed upon the sensations of position of the limbs and appears, as such, in the form of what is known as sensations of the movement of the limbs. These two groups of experiences are usually carefully separated from one another, as if there were even a qualitative difference between them. For this reason they are both known as kinds of sensation, whose differences presuppose the existence of different kinds of sense-organs. In favour of their separation,



it is argued that sensitivity to movement varies from part to part of the body, but does not run parallel to the sensitivity of these parts to their position. Thus the movements of fingers and toes seem to be felt equally well, although we are hardly conscious of the position of the latter (10, p. 751). In favour of their identification through the medium of the modification of motion, the following considerations have to be urged. (1) It is a familiar fact that in the sense of vision and more especially in that of touch, the discrimination of simultaneous points is very much less acute than is the sensitivity to a moving stimulus. The sense of position, in touch and in vision, or the sensitivity to the mere presence of a sensation may also be said to be much blunter than the sensitivity to movement, especially if the stimulus whose position has to be observed has been acting steadily for some time and is accompanied by others. Let it move even very slightly and it will be noticed at once. (2) Both the sensitivity to position and the sensitivity to movement vary in different parts, but not concomitantly<sup>1</sup>. It is evident, therefore, that the objective disparity between sensations of movement and those of position is not greater than that between a moving touch<sup>2</sup> or sight and a simple sensation of these kinds. (3) From the subjective side, it may also be said that there is quite as great a difference between a steady visual sensation and a moving one as there is between sensations of position and sensations of movement. It is clearly an easy matter to show that both visual sensations and visual motion are dependent upon the same sense-organ, but there are obvious difficulties in the way of the accurate physiological identification of articular sensations of position and movement. We are therefore thrown back upon psychological comparison and analysis and there can, surely, be no doubt that in the light of the considerations just put forward the physiological

<sup>1</sup> Cp. 10, p. 366, "Die Wahrnehmung von Bewegungen an der Netzhautperipherie ist nach Exner und Aubert viel feiner als das Distinktionsvermögen daselbst, und Exner schreibt den peripheren Netzhautpartien geradezu die Rolle zu, Wahrnehmungen von Bewegungen zu vermitteln." It is therefore evident that any difference of effect produced by faradisation of a joint upon the thresholds for articular position and for movement cannot be brought forward as an argument in favour of the qualitative distinction of articular position and movement. In fact, the greater blunting to position is quite natural.

<sup>2</sup> Such expressions are used deliberately. Seen from the level of perceptual integration, they are of course insufficient. They would then become "a moving tactual stimulus," etc. From the sensational level, with which we are here concerned, "a moving touch" is correct and unambiguous. In strict psychological sense, there never can be any confusion of stimulus with sensation or the like, but only of one level of integration with another.

and psychological independence of these two classes of sensation would constitute a gross extravagance of sensory mechanism<sup>1</sup>.

We are therefore confirmed in our previous opinion (p. 140) that the sensations of position from one joint, or from various joints for that matter, are to be considered as differing in order. The derivative nature of the sensation of position is sometimes supported by reference to the fact that we gradually lose a clear sense of the position of the arm if the attention is distracted and every movement and contact of the arm with other parts of the body is prevented (cp. 30, p. 155); the sensation of position, it is held, is only an after-effect of that of movement. But such an argument is worthless. The facts can be explained by a theory of adaptation similar to that commonly accepted for touch, that pressure is only felt where there is a quick change of pressure over a given area (8). The facts, therefore, support the primacy of the articular movement as little as that of tactual movement, as against the simple sensation from the "spot." Psychologically the facts may indicate the presence of the aspect of intensity in articular sensation. A semblance of extensity seems to be given in the different voluminosity of the sensation of movement from the thigh compared with that from the little finger. We should then have the full complement of attributes in this sensation, all of which, however, owing to the peculiar physiological conditions of the case, are much clearer and more easily observed in the complex of movement than in the single elementary sensation of position.

*Labyrinthine.* Our awareness of the motion of the body as a whole may also legitimately be conceived as a form of motion and as based upon sensations of position of the body as a whole. This view is also opposed to current theory, which treats the two kinds of experiences as different kinds of sensation. Physiological investigation supports the latter in so far as two separate sets of sense-organs are found, one for each group of sensations. But this is only apparently a difficulty. For it is well known that the various parts of the skin and of the retina, which contain very frequent repetition of the same sense-organ, are not

<sup>1</sup> The physiological problem of the sensory mechanism, of which at the present time we know next to nothing [cp. 30, p. 25], is in this case, as in all others, quite irrelevant, for it is quite possible that it consists of a very complicated form of physiological integration. This is unimportant to psychology, so long as the sensation evoked possesses the full number of attributes, including order. It would, on the other hand, be a highly important fact for psychology, if it suggested to us the lines of psychological integration. We find a physiological integration, for example, in the labyrinthine organs of position and movement.

equally sensitive. Again, we find different systems of sense-organs in the skin, which provide us with very similar kinds of sensation whose peculiarities show variation; the sense of temperature, for example, is based on a protopathic and an epicritic system, of which only the latter shows the process of adaptation. So too in vision do we find different kinds of sense-organs procuring very similar sensations, which differ, however, again in regard to the process of adaptation or special sensitivity to certain degrees of light. It may be agreed, then, that a reduplication of sense-organs giving the same primary experience, whose actually realised complications vary somewhat in character, is quite a usual occurrence.

It may be taken for granted that the sense-organ connected with motion of the body as a whole is a special device for obtaining sensitivity to all acceleration of movement, so that the organism may adjust itself to the change. This sensitivity to acceleration of motion can only be obtained if the change brought about by any acceleration is removed as quickly as possible, so that the organ may be highly receptive to any new acceleration. The organ of position, on the other hand, must be specially sensitive to position as against movement. An organ which has to be stimulated continuously by the fact of its having taken up a certain position could hardly at the same time be one which responds delicately to even an incipient change of speed of motion. For the readjustment of the organ to motion might very well be taken for a readjustment to position and *vice versa*. Their separation, therefore, becomes a matter of necessity. The provision of a large sensitive area, such as the skin, in part of which a motion-complex could be produced and set in order-relations to sensations from other parts, would not obviate the necessity for separation. For the stimulus to sensations of position and movement of the body as a whole must surely affect the whole body and, therefore, the whole specially sensitive area at once. If the whole skin at once were always affected either by constant or by moving pressures, our tactual would closely resemble our present articular sensitivity. For we should then be keenly sensitive to waves of motion passing over the skin, but we should quickly lose our sense for them, when they came to rest and acted continuously on the same elements of the sensitive area. Creatures endowed with our sense of touch, who lived in a fluid medium which never moved over them except in continuous waves passing from head to tail and which never exerted steady punctate pressure stimulation upon them, would never experience anything but touch-motion. There must, therefore, be specialisation as



well as separation of sense-organs for position and movement of the body as a whole.

What, then, are the attributes of these sensations? Sensations of position do not seem to be capable of variation in intensity or extensity. We can therefore have only the vaguest, if any, introspective appreciation of the actual degree in which these aspects are given, and we can make no use of them in experience, if they do not vary. For integration with an unvarying element could not render an ambiguous complex of sensory data unambiguous. But of variations of position we are definitely, although not often in isolation, aware. The question therefore arises whether these variations are variations of quality or of order. For several reasons it would be more acceptable to call them variations of order. For our sensations of position do not seem to differ in quality. How should we be able to treat them all as sensations of position, if they differed in quality? Or how should we come to arrange them for our use into a system of interrelated positions? If they differ in order, however, the basis of their arrangement and of their use is at once given. Mere introspection can hardly lead us further than this.

When introspection fails, we must have recourse to a comparative study of the forms of integration in which labyrinthine experience occurs. An examination of these must show us how it enters into combination with other sensory experiences and what new feature or modification of experience results therefrom. If even then we are not quite clear of our difficulties, we must resort to general principles of integration, derived from an examination of the manner of integration of sensations whose elementary characteristics are familiar to us. Now we do find cases of the integration of the order-aspects of sensations, whether these be qualitatively the same or different, while we have no good example for the occurrence of an integration of the order-aspect of one sensation with the quality of another of the same or of a different kind. We shall, therefore, assume, for the present, that labyrinthine sensations vary in order.

It must, however, be noticed at this point that the psycho-physiology of the labyrinth is entering upon a critical stage of its existence. It is on the one hand, a matter of doubt whether the vestibular nerve has any direct connexion with the cortex (2, pp. 78, 91), and it is asserted that the existence of vestibular sensations proper is not proved (*ibid.* p. 91); on the other hand, there is evidence that voluntary inhibition of nystagmus does away with the sense of bodily rotation, not merely after the rotation has stopped, but also during the actual rotation

(5, 13). It may be shown in time that our labyrinthine motion is a modification, resulting from a more or less complex process of integration of visual or, in the broadest sense, pressure-sensations or both. It must, however, in any case remain the modification of motion it is and be amenable to the line of treatment here advocated. Its difficulties and problems offer no particular obstruction to our theory, which will, on any showing, probably be right in the main principle.

*Olfactory.* We can point to nothing resembling motion in the sense of smell. The attribute of order is not patent in olfactory sensation. Presumably there are in this sense no neighbouring sense-organs of the same kind. If there are, the attribute of order has been so integrated with others that it is at present unrecognisable. Probably the very slow rate of change possible with olfactory stimulations precludes the realisation of the integration of these hidden differences of order into a motion-like modification, which would, as such, be readily noticeable. For in all other senses the rate of change of order which constitutes motion must not fall below a certain minimum. We are here faced with problems, not with radical exceptions or difficulties.

*Visual.* Motion is visual, *par excellence*. If the primary visual sensations are well marked and in sharp contrast with one another, as are *e.g.* those from a broad black strip upon a white ground, motion of the black strip can be detected at some 50—60 sec. of arc per sec. of time displacement. The limit of distinction of visual points from one another is found when these subtend an angle of about one minute. On the retina this angle would allow one unstimulated visual element to intervene between the two excited by the points seen. Higher visual acuity than this is rather exceptional and is very difficult to explain visually without the help of eye-movements, whereby the increase of sensitivity may possibly be obtained by a movement of the eye allowing one and the same visual element to be stimulated by the two points successively or, less probably, by the kinaesthetic sensations afforded by the eye-movements as such (10, pp. 346 f.). As the minimum angle for the detection of motion is smaller than that for the detection of distance, where only one intervening point is presupposed, we may at least assume that visual motion supervenes upon the successive stimulation of two sensitive elements of neighbouring order.

*Time-limits of motion.* Change of order must take place at a certain minimum rate, if motion is to appear. This is most familiar in vision, for which the limiting value has just been given. With slower speeds the motion only appears after some seconds or not at all. In this

respect motion behaves quite like the attributes, *e.g.* intensity. In the simplest form of pressure-motion, when the stimulated surface is quite at rest, there is also a minimal rate of displacement, which has not yet been determined exactly (10, p. 722). For articular sensation the minimum rate of displacement varies from  $0.25^\circ$  in the hip-joint or  $0.3^\circ$  in the shoulder-joint to  $1.4^\circ$  in the ankle-joint per second (10, p. 753). The range of speed of displacement throughout which motion is appreciated is very great; in vision the highest limit is some 24,000 times the lowest limit (10, p. 368). The threshold for the perception of motion is the higher, the farther towards the periphery of the field of vision the stimulation takes place.

*Speed.* The rate of change of order appears in motion in the form of speed. Speed is measured by reference to the distance traversed by the moving body in the unit of time. In experience, however, we notice differences of speed without any conscious reference to distance or time and without any medium of comparison. Motion as a modification is not more truly motion with a fast speed than with a slow one. It is always just motion. Its form of variation is speed, which, however, we can measure only in the way we can measure other variable modifications of experience, by relating stimuli to just perceptible differences. Judgments of speed are, therefore, based upon a direct criterion, present in experience (17, p. 374).

*Order-difference limits of motion.* No real motion of an object is necessarily presupposed by a moving sensation. Change of order, as defined, is alone requisite. But this change need not progress strictly from one order to the next neighbouring. A considerable change of order is compatible with the effect of motion. Certain stages of the motion may be omitted without spoiling the effect. Upon this fact the familiar apparatus of the wheel of life or the stroboscope and of the cinematograph is based. A succession of pictures of an event, each of which, of course, is entirely devoid of any movement or displacement, is projected upon the eye and is seen as a perfect representation of continuous motion. A series of small electric lamps set at a certain distance from one another, which can be lit and extinguished successively, serves to demonstrate this fact in its simplest form (19, pp. 60 ff.). The continuity of the motion is broken if the time or space intervals between the lights exceed certain amounts, which are to some extent interdependent; but the effect of motion is not suppressed unless these intervals are much larger. If the time-interval between the lights is decreased beyond the value for continuity, several of the lights become



visible at once, each one being in apparent motion. A full psychological definition of motion states, therefore, that motion is the unification of successive differences in order of sensations which follow one another within a certain range of time-intervals. This range is determined by the degree of the difference of order of the sensations, which may not exceed a certain amount. The introduction of intervals without the omission of phases means a slowing of the motion which results; the omission of phases is followed by no marked effects, until the interval reaches a certain amount, when the motion becomes jerky and interrupted. Although the effect of motion is still distinctly present, the single sensation or picture can be distinguished more and more as the time-interval increases. The modification of experience which results from this integration of order-differences may be described as unitary and progressive change of order.

*How do the primary sensations integrate to form the modification?* There seems to be no valid reason why we should not say that, when two or more sensations of position of neighbouring order are evoked successively at a certain interval, they unite to form the experience of motion. Conversely, we may assert without fear of serious opposition that two or more sensations of position are given psychically, when a corresponding experience of motion is evoked by the successive stimulation of two neighbouring sensitive elements. We cannot object that no sensations are distinguishable in the integrated states. For we could not expect these sensations to be distinguishable, so that we might discriminate them one from another. It is just because they integrate to form a unity, that we have any such state as motion at all. To prove a fusion of particulars to unity we do not need to show a temporal process whereby discrete particulars have come together into unity. And we do have a multiplicity of sensations in this immediate unity, in so far as we realise it in its own inherent character—change of order.

It is important to mention a number of ways of stating or explaining the connexion between the integrating sensations and the resulting modification, which have been put forward for one or the other modification of experience. The consideration of these statements may seem very pedantic and forced in relation to motion. But it is just because of this that we would repeat them here; for if they are inapplicable in the case of motion, we shall learn to dismiss them here and shall understand their invalidity in cases parallel to motion, where they have seemed to be of genuine worth.

Our past experience is often considered to have an important influence. It might be said that, having often experienced two tactual or visual experiences successively in a complex of circumstances which otherwise led us to know that a stimulating object was moving over against us, we have come to know that these successive sensations mean motion; so that, when they afterwards occur without the complex interpretative circumstances, we yet know from past experience that they mean motion. Or it may be maintained that some inner power of thought operates upon the data of sense and extracts from them certain meanings, previously implicit in them, or unites with them to form a state of meaningful perception. Or it may be claimed simply that our experience grows from within and blossoms out into these modifications. It may even be said in abandonment of all problems that the mere juxtaposition of the data of sense is all that we ever seem to experience or do experience; there is no new, nameable modification of sensory experience at all; from beginning to end we have only sensations in juxtaposition.

Most of these "explanations" are empty, because they do not state how the result actually obtained is brought about by the means alleged. The mechanism of the operation is left in entire obscurity. How, for example, should past experience be capable of all it is supposed to do? If we only mean to state the fact that our present experience is dependent upon experiences we have had, we must be at pains to state that we do not know the mechanism of this dependence. We must also attempt to discover its specific nature<sup>1</sup>. Not only motion, but all secondary sensory modifications—melody, distance, interval, depth, apparent size, position, distance and depth, tonality, and all the *nuances* of perception—present the same problem: by what means does it come about that the presented appearance of sensory data changes with circumstances? These many and various changes cannot be adequately explained by a reference to the knowledge we have gained of the approximate real nature of the objects which evoke them, or the like. For it does not appear how the significance of any knowledge we may have gained should actually change the appearance of our sensory experiences as they present themselves to us. It can be

<sup>1</sup> As Stumpf says (25, Vol. II. p. 195): "Wenn die Kraft, welche allein Verschmelzung bewirkt, wegfällt, wird der Effect ebensowenig eintreten, als die Locomotive aus Gewohnheit läuft, wenn sie einmal nicht geheizt ist oder...dem Kurzsichtigen, der sich eine Brille anschafft, nun etwa gewohnheitsmässig immer noch alle Umrisse ineinanderlaufen." So too, of course, for any secondary modification of experience and any extra-mental influence.

easily recognised that knowledge has not this penetrating influence in every case. If it is shown for any case that cognitive states are the effective influence, we must also be able to show how they produce the change in question. If we can show that the sensory data themselves suffice as an explanation, then we can dispense with remoter influences, whether these accompany any changes regularly or not. For it is not at all unlikely that knowledge in many cases is dependent upon sensory changes and not *vice versa*. In any case, it is impossible to work with the conception of transcendence, whereby a complex state of mind derives its appearance in part from influences which are not given psychically at the moment. We have already noticed that we cannot carry our demand for a causal explanation further back than the elementary data of experience; but we must be able to reduce our whole experience to these and to explain it fully without appeal to any other data. For, as it is clear that all our knowledge has been gained from our experience, it is not intelligible how our experience should reveal what it is not yet affected by. For experience can only reveal that which modifies it. If experience shows any change, there must be some new datum present responsible for it. We cannot expect to explain the simpler in terms of the more complex, but the contrary. We must therefore find all the elements of experience and attempt at least to explain all experience in terms of these.

Our only possible conclusion, therefore, is that a moving sensation consists of at least two primary sensations as such and in so far as they are not the same in respect of order; so that the two together present a change of order, that is motion. The further difference of increased extensity which they also present, we are not at present concerned with. It might, however, be urged that there is no apparent way in which two pressure-sensations could come into such close union that their differences of order might form a new unitary modification of sensation-complex whose elements do not seem to be individually segregated. But our definition of sensation sets no limits to the boundaries or affinities of sensations to one another. The presence of extension as an attribute may logically, but does not psychologically, presuppose the existence of limits to that extension, which, as we see in vision, are only got by virtue of a quick change in quality, *i.e.* by contrast. In touch a boundary is given by a special emphasis on quality, where the change in pressure is rapid; no sharp limit is given thereby, but only a certain amount of extension (22). We may therefore confine ourselves to saying upon what occasions sensations



do actually fuse their differences in respect of any one attribute into some new unitary modification.

Motion is not based upon any conscious comparison of the order-aspects of the first and last or of these and the intervening sensations. Nor is there any unconscious inference from these. It is simply the integration of the differences in respect of order of the given sensations. Nor can we analyse the experience of motion into a series of sensations of position. We know the positions a flying arrow has occupied, but we cannot separate out in sensory experience the unit-sensation of any one position. For where motion is in experience, there never is merely a number of different positions, but a series of positions which unify to form progressive change of position. Motion is not merely a way of speaking of or a name for a number of positions. It is a new modification, which though based on sensations of different order, is more than these, because it is a unity of them. It is a difference of position<sup>1</sup>, based on given orders and integrated from them immediately in the way characteristic of experience. Our point of view, therefore, cannot be called sensationalism. Nor is it that view which looks upon every new unity of experience as a unique, irreducible element. It contains both of these positions in itself and finds a partial truth in each.

The sensations upon which motion in any particular case is based are not lost in the resulting experience of motion. We do not propound a kind of mental chemistry, as that was understood by early British psychologists. For the experience of motion, though new and unique, supervenes upon the quality of the sensations given as an integration of their order without thereby changing their quality so as to make them in any way unrecognisable. Nor are the extensity and intensity of the integrating sensations necessarily changed in the least, although they may be so slightly according to circumstances, when these operate upon them. This is a point of view which must be maintained throughout the whole treatment of mental modifications.

*Motion and the Attention.* Motion is said to exert a strong attractive power over the attention. But we need not yet appeal to remoter powers such as that of the attention. In an otherwise resting field of cutaneous, visual or auditory sensations, a moving sensation is not merely one among others. It is one like the others, of course, but it

<sup>1</sup> Here is the inset for one of the central problems of philosophy, how the mind knows differences together. This is first of all a problem for descriptive psychology. It must not, however, be confused with our issue, which is concerned with what results when differences are given together. The problem of knowledge is quite another.

is characterised by a peculiar modification which the others lack, and having this feature of motion it behaves in our mind as would the sight of a single red rose in a bunch of white ones, a single light in the darkness or a single sound in the silence. That our attention is drawn to each of these things, means simply that only one of a peculiar class of experiences is presented in each case. The separation for the attention is given without the help of the attention at all. To this peculiar isolation, which the presence of a mental modification may give to a sensation, we have, of course, to add the peculiar sensitivity which is represented by the much lower threshold for successive than for simultaneous discrimination.

The attention may be directed upon any of the phases of a motion generally. But in particular instances, it is very much easier to isolate certain phases than others. This fact accounts for the conventional representations of men and animals in motion and especially in rapid motion. The most prominent phases are, of course, those at the beginning and end of any motion or at a change of motion, where vision obtains the advantage of the slightly longer duration of these phases. These positions, once made familiar in art and illustration, help to fix the attention of those who study them, so that they are seen regularly and are used to suggest or symbolise motion. The strange positions which men and animals occupy when in motion, as revealed by modern photography, are observed for the first time by everyone with great surprise. Naturally they seem very ludicrous, because we never do see animals or men in these positions unless they are in motion. To see them in these positions at rest has the same queer effect as the sight of a person suspended in mid-air, as if comfortably at rest upon a couch, would have. We see them without that conscious modification which alone supplies the key to their interpretation. The difficulty we experience in isolating these phases of motion in the attention really shows us the attitude of attention towards motion. When many motions are given together, the attention behaves in the same way as when many sensations of any kind are given together. No one would suggest that when many motions are given, the attention to them all is raised consistently to a higher level. Attention to motion, therefore, is rapid when only one motion or unitary complex of motions is given. Then the attention behaves as it does to any peculiar and unique object. When there are many motions, the attention acts towards them as towards any group of similar experiences, sights, sounds or thoughts. It even finds it particularly hard and embarrassing

to follow one among many motions, until it is trained to it, and it will overlook one movement of importance among many others of a similar kind as readily as it will overlook one of many motionless objects.

In attending to motion, the attention must in the first place be directed towards the moving sensations. We may express this better in accordance with actual speech by saying that we attend more to the things that move than to their actual motion and that we cannot abstract their motion from them entirely, so as to separate the one from the other. For the present, however, we must attempt as far as possible to avoid the phraseology of knowledge, for such modifications as motion do undoubtedly occur before there is any clear evidence of the occurrence of knowledge. The matter may, perhaps, be best stated by saying that the modification of motion cannot be separated from the basis of sensation upon which it rests. No motion, we may assert, ever occurs without the simultaneous occurrence of primary sensations. The connexion between these two things is, however, psychically much more obvious than this. Motion is psychically always *attached* to primary sensations. This fact it is which has led, as we may now say, to the hypostatisation of a class of sensations of movement of the limbs. Obvious though it be, it is important to emphasise here, that a modification such as motion cannot be experienced alone or attended to alone in separation from its basis in primary sensations. Apart from such separation, it may be attended to for any length of time allowed by the continuous operation of the sensory stimuli to the primary sensations which carry it. For, as has already been indicated, these sensations, and with them motion, are adequately conditioned by the stimulatory complex and the ensuing integration, apart from all higher processes of integration which may be implied by attention. No modification of experience can be separated or detached from its integrative basis, so that the observation of the former is dependent upon the continuance of the latter. If the integrative basis of a modification is itself dependent upon the attention, the resulting modification will of course be destroyed if the attention is directed upon it.

*Melody.* Melody is based upon tone-sensations which differ progressively or within certain rather indefinite limits in order or pitch<sup>1</sup>.

<sup>1</sup> There is, in sound, another form of motion that stands for change in the place of origin of the sound-stimulus. But that is obviously a derivative of the localisation of sound, which again is dependent upon intensive differences. The nearest relative of visual motion is, therefore, melody. The spatial motion of sound resembles the integration discussed in the text in many ways, but it cannot be dealt with here.



There can be no doubt whatsoever about the introspective similarity of the two modifications; which seem different only because one is change of localisation and the other change of pitch. Pitch *moves* in a melody. A succession of tones of different pitch which does not move is no melody and can be realised only under certain circumstances of time- and pitch-interval. A melody is not merely change of brightness, nor is it merely meaning or emotionality, although it may also be these at any time. It is essentially a unity and progression of pitch.

All the psychological characteristics of motion may be transferred to melody. The minimal order-difference which will constitute melody, as all the physiological theories of hearing suppose, is the passage of a stimulation from one sensitive element of the ear to the next neighbouring. As in vision, so here also change of order must take place within certain rate-limits, if melody is to be appreciated. An upper limit of melody, as of motion, is only given by the possibilities of the physiological process of damping of the resonators of the ear, or of the equilibrium of forces at the sensitive element. Melody therefore also varies in a characteristic way by its speed, or by the interval which it compasses in a given time, although this is very much affected by the simultaneous change of voluminosity, which adds to the quickness of change a certain difference of brightness and lightness or sombreness and weight, or, it may be, also an emotional sense of gloom or gaiety. Melody, like motion again, is restrained within certain limits of successive order or pitch-differences. The continuity of melodic progress is not markedly affected by the introduction of a pitch-interval between two immediately successive tones, as in *legato*-playing on an instrument with fixed tones. With certain rates of succession of tones it seems to be perfectly continuous in its progression<sup>1</sup>. With slower rates it seems to rest at each tone for an instant and then to spring to the next following, while with higher rates we hear several tones together. Beyond a certain, not very definitely fixed, interval our

<sup>1</sup> Certain pathological conditions may very much increase the maximum pitch-interval that may separate successive tones, which, played at a certain rate of succession, seem to form a perfectly continuous progression of pitch. In the case described by Grant Allen (3) this interval even in the middle octaves was as great as a third. These pitch and time intervals and the whole introspective problem of melody have not been investigated experimentally, as far as I am aware. The statements of the text are based only upon general observation, but are easily verified. It is significant that Stumpf (Vol. I. p. 185), against the view of Grant Allen, who compares this increase of the critical pitch-interval "properly" to loss of quality in vision (*i.e.* colour-blindness), finds in the facts a greater resemblance to pathological cessation of function of *parts* of the field of vision, *i.e.* disappearance of certain "orders."

sense of melody is not aroused. This limit we reach approximately with the octave. Nor is melody affected by the introduction of a time-interval between the successive tones, as in *staccato* playing, provided that it is not too large. Here again the limits have not been precisely ascertained.

It is therefore evident that, on the whole, the musician's use of the words motion, line, curve, wave and the like in relation to melody is, from a psychological point of view, perfectly justified. Obviously it is no mere analogy with vision or with the arts of vision which prompts the use of these terms, but rather introspective familiarity with the motion-like nature of melody, its smooth continuity or jerky abruptness and its evenness or variation of speed. In this connexion, the usual means adopted to increase the motion-like progression in melody are interesting. The player often dwells very slightly on one or more tones to the disadvantage of a few following, which have then to be got into a slightly less time, so that in them the speed and therefore the continuity of progression is increased. In exaggerated form this is the familiar *tempo rubato*. A proper grading of intensity will also often accentuate motion. The composer has the obvious means of multiplying the number of intervals of a unit-size passed in the bar, which heightens speed, the introduction of continuous or chromatic passages, which increases the smoothness and continuity of motion or line, *legato* indication, and in *legato* passages the variation of the number of tones passed in each beat, which, by varying the motion, makes it more prominent.

Melody also offers itself with the same ease and difficulty to the attention, as does motion. If pitches can be distinguished at all, it is impossible to overlook a melody upon a background of consonance which does not physically overwhelm it. It is difficult to follow one melody amongst several, unless the tones are marked out by some constant feature, *e.g.* highest pitch of tones sounded simultaneously, a certain timbre, as when melody is played on one instrument amongst others, a certain intensity or the like, as when several voices are played on one instrument at once. It needs practice to follow several melodies together. An isolated part of a melody is as bizarre and meaningless as is part of a motion. If anything, it is the beginning of each which is most typical and representative for imagination and recall.

Melody is inseparable from tones, to which it is always attached. It cannot be recalled apart from them and is therefore ever experienced anew. Properly speaking, we should say that a series of sensations is

revived which integrate to melody. Of course the melody may be the real object or aim of recall, but nevertheless the integrating tones are the mechanism of this recall. If we make the continuance of the integrating tones dependent upon the attention, it is impossible to attend to a melody without destroying it. If melodies are not separately reproducible, neither do they leave an image behind, nor can they associate with one another or with images. Melodies have no intensity, voluminosity or localisation, apart from the tones upon which they are based. The variation of their constituent tones in voluminosity gives them, as already noticed, a varying character of brightness, besides that of "speed" native to them. Their other qualities come from other forms of integration.

It is often said that melody presupposes one or other thing, such as rhythm, consonance, interval or tonality. But after our consideration of motion as a modification, we may conclude that melody presupposes nothing not included in its definition. It is possible without tonality or consonance, as in the birds. Its intervals may be most indefinitely fixed, as in the first cooing of a child. It may or may not always be psychically concomitant with rhythm; it is at least in no way dependent upon it.

## § 7. II. *Distance.*

The next modification of experience in order of simplicity and the nearest allied to motion is distance. When a motion of some extent occurs, we do not recall at the end of it where its beginning was and infer the amount of its course therefrom; we have rather a direct experience of the amount of the distance. This direct experience, like motion, is independent of any conscious comparison of the order-aspects of the first and last or of the intervening sensations. The experience of distance is not composed of sensations of position; nor is it the imagination of the extended pressure of an object stimulating the extensity intervening between the two points touched (*v.* 29). It is based on the differences of order of certain sensations of the same qualitative class. Nor is there any conscious or unconscious inference from the two end-positions. As we have already urged for motion, so we would argue for distance, that the difference in order of two or more sensations of the same quality constitutes a distance. No one assumes the existence of a class of sensations of distance. Distance is generally recognised as a perceptual result, but such a classification clearly raises it much too far above its real sensational basis.



Distance must be carefully distinguished from motion. An approach to progressive difference of order is essential to motion. Beyond certain limits of difference the motion tends to disappear. Distance is not confined by these limits, so long as the two or more constitutive sensations are not restrained by one or other circumstance from free integration. Even two points at the limit of order-differences may constitute a distance. Successive occurrence of sensations is presupposed by motion, but not by distance, which is only restrained by too great a time-interval between them. Though the limits of this interval have not been fixed, it is clear from experiments already done that the time-limit for distance is much greater than for motion. Motion is within its limits the integration of successive and progressively continuous order-differences. Distance is within its limits the integration of any simultaneous order-differences. The limits of motion are set by degree of order-difference and by time-interval. The limits of distance are set only by time-interval. Distance may therefore occur apart from motion when the integrating sensations are given simultaneously. It is naturally more distinct in this form, since observation may be directed upon it as long as it continues or for any length of time. It is, on the other hand, often more urgent and clearer when it accompanies motion, for being clearly delimited by the progression of motion, its objective basis is thereby already unified and therefore always unifies to distance as well. Besides, two modifications are more effective than one. If there is any rivalry of distances, that characterised by motion will be more effective.

We find distance in all those senses which show order and are capable of the modification of motion. In the sense of pressure it has been treated experimentally in an exhaustive manner in the discrimination of points touched on the skin. This is the very familiar aesthesiometrical work. In vision extensive research has also been carried out involving the comparison of lengths of lines or of distances between points. Distances traversed by moving limbs have also been carefully studied. Only in hearing is the modification of distance less familiar under this name. There it forms the familiar phenomenon of interval. It can hardly be disputed that as a matter of fact we are in some way aware of the extent of movement or of translation of the body on the basis of labyrinthine sensation. We cannot expect to have a fine sense of distance in this particular quality of sensation, for, as we have seen, positions are here not given in isolation. It is therefore as impossible to separate single positions from the continuous motion

here, as it is in continuous visual motion. Labyrinthine distances are thus appreciable and comparable, but they cannot be accurately fixed or subjected to conceptual treatment.

That distance, like motion, is constituted not by orders or by sensations of position, but by difference of order, is borne out by many facts. The chief of these is the ease and accuracy with which extents of movement and distances can be noted and compared, even when the end-positions of the distances compared are different. We may easily remember a distance or motion without remembering the position of its limits<sup>1</sup>. All the facts concerning the variation of apparent distance under certain circumstances also bear this out. "When a movement is freer and easier than an other, and so produces a less sensation, it is underestimated with regard to this other and tends to be prolonged" (30, p. 109). The apparent distance of a movement is also affected by fatigue, slowness of motion and attention, which make a movement appear longer than it otherwise would appear to be (30, p. 109). Each of these influences has the effect of making differences of order seem greater than they really are, because the difference of orders of the end-positions is distorted and not these orders themselves, as they are psychically given. This distortion of differences is doubtless great where distances are given by means of motion, for then the temporal lapse of the first sensations leaves nothing to guide the judgment except the modification of distance; but the same kind of distortion is possible in distances given in simultaneous stimulation, especially when distances proper and not end-positions are compared, as in the comparison of short lines as such. Distances seem greater at one time than another, merely because a variable modification of experience like distance is a direct psychical datum which arises under the same conditions as *e.g.* intensity. The distortions of all modifications of experience by various influences seem to have a common nature. They would have to be considered systematically, in connexion with the illusions.

*Threshold of distance.* For the discrimination of successive stimulations this is nearly always somewhat, and sometimes very much, lower than for simultaneous stimulations. Two touch-spots stimulated

<sup>1</sup> Cp. 30, p. 155. The systematisation of the facts suggested by Woodworth makes the path of research seem infinitely long and completely excludes any gleam of daylight from it. On the other hand, the one here proposed has all the merits of a system. The facts arrange themselves in it willingly and form profitable knowledge. The whither and where of surrounding facts also become clear and violence is done to none.

successively are distinguishable from one another when they constitute neighbouring sense-organs, while simultaneous stimulations need to be many times as far apart from one another to be distinguished as two. The origin of this peculiar fact is to be found rather in physiological than in psychological conditions. It is usually explained by supposing that the stimulation for each sensitive point radiates over a certain area round its most intense effect upon the cortex, so that the edges of two areas excited simultaneously from two neighbouring points often overlap and produce either one maximum or a level, until they are so far away that the sum of the edges where they overlap is not equal to the maximal part of either area, and these therefore form two points of maximal excitation. This explanation is supported by introspection, which shows that when the distance between the two points is increased, the stimulation is felt first as one point, then as an increasing oval or small line and then as two separate points. The distance in the oval or between the two points increases rather markedly as soon as the points stimulated begin to be differentiated as two or as an oval. It is therefore evident that if we are to attribute any systematic psychological importance to the fact of thresholds and their variations, we must, in the case of touch at least, hold rather to that of successive than to that of simultaneous stimulation.

The facts are much the same for vision; one unstimulated sensory element must lie between two that are stimulated, if these are to be distinguished. Otherwise the two points are felt as a short line or oval. With the help of successive stimulation or eye-movements, the threshold for the psychical realisation of order-differences and therefore of motion and distance may be reduced to the lowest possible limit, to that of neighbouring sensitive elements. So in articular sensation, we are able to distinguish short movements, before we are able to discriminate from one another the two end-positions occupied by the limbs. We may therefore maintain generally that the modification of distance is present as soon as a difference in the order-aspects of two successive sensations is given at the proper interval of time. These intervals are not known to be different from those indicated for motion.

*Direction.* Short distances are therefore perceived before the points bounding them can be distinguished from one another. Only when the points stimulated are some little distance from one another can they be distinguished as discrete. The same holds for the appreciation of the direction in which two points lie to one another; for



this involves a quite clear discrimination of the order-values in at least two points of the line formed by the end-points. Awareness of direction seems to be an experience which involves higher forms of integration than motion and distance. Appreciation of distance, on the other hand, is based on the psychical presence of order-differences and involves no discrimination of positions, as in cutaneous, visual and articular sensations. In labyrinthine sensation direction is said to be distinguishable as soon as motion is felt at all (10, p. 750). We have already noticed that we have here no means of distinguishing end-positions. Possibly the peculiar composite nature of the sense-organ has some determining effect here.

The apparent distance separating two points varies with the threshold for their discrimination; the higher the threshold, the smaller will the distance seem. This relation is doubtless based upon the unequal number of touch-spots at various parts and the consequent unequal representation of various areas of the skin upon the cortex. There is no reason to suppose that the physiological separation of two areas of excitation on the cortex should vary much from part to part.

*The variation of distance.* A form of this is given in the greater or less distance that may be integrated from the differences in psychical order of the constituent sensations. Judgments of distance, or, as they are often called, of extent, are therefore, like those of speed, based upon a direct criterion present in experience. So *e.g.* movements of the arm may be and will usually be judged as to their extent directly by mere reference to the modification of distance which ensues. That this should have been denied in favour of duration as the basis of judgment<sup>1</sup>, can only be accounted for by the fact that opinion generally separates sensations of position and of movement into two different classes. But if sensations of movement are supposed to be elementary, it is, to say the least, unusual to suggest that they are primarily qualified by an aspect of extent of this unique kind, so different from the usual extensity. On the other hand, these sensations can hardly be supposed to have an aspect of extent, for their supposed derivatives—sensations of position—do not show much of it. In place of extent, therefore, duration is the only obvious sort of attribute these sensations have to show, and even that can hardly be called obtrusive in sensations

<sup>1</sup> Cp. 17: "The comparison of the length of arm-movements is made through the comparison of the duration of one or several of the sensations arising from the movements (preferably the joint-sensations) and of a particular value of the joint-sensation, called here the rate-value." For experimental data against this view, cp. 12 and 30, chap. iv.

of position. Duration and extent may, of course, be distinguished in sensations of movement after a fashion; where extent is the distance in our sense and duration the time taken to move the arm through that distance (21). But if extent can vary, surely order, or perhaps even quality, should also be variable, which does not seem to be assumed in this case. The only convenient and at the same time the obvious way out of these difficulties is to connect the two groups of sensation as we have done, and to see that sensations of movement constitute the modification of motion and distance for sensations of position, which then have the full complement of attributes necessary for the judgments based upon them, viz. quality, order, extensity, duration and intensity. We can then readily allow<sup>1</sup> that motion, the speed of motion and distance are all specifically perceived, while the duration of motion is as directly given in experience as is any duration. The same is true of the order of any sensation or of the general character or change of character of any motion, *i.e.* of the position of a movement.

There is a very great difference between the true comparison of distances in introspection and the comparison of lengths of line by laying one alongside the other. In the former we compare with one another the differences between two pairs of orders; in the latter we compare single orders with one another and infer from the result the comparison of the intervening distances. These two processes are both possible, because distance, like extent, is based upon the order-aspects of more than one sensation. It is therefore possible to turn both the primary and resulting secondary modifications into amounts or quantities by the identification of the orders of the elementary unit-sensations. Motion can also be treated in this way and is actually measured for physical purposes for the identification of points passed in a unit of time. But such measurement is not usually possible to the unequipped eye, except in the case of the modifications based on simultaneous data, such as distance and extent. It is so easy and advantageous to measure in this way that we have constantly to be on our guard against it in experimental work. No one relies solely upon the comparison of distances as such, where comparison by identification of orders is at all possible. If we wish to obtain comparison of distances, we have to use a method which will prevent the identification of orders. Under these circumstances we find that results conforming closely to Weber's

<sup>1</sup> With Woodworth, 30, pp. 150, 169 f. Woodworth, however, gives no clear indication of the basis of these different perceptions. With him too we may readily allow direct "judgments" of the force used in, and the resistance opposed to, any movement.

law are obtained for short lengths of line. The law does not hold, however, throughout a large range of distances, because of the ready applicability of the quantitative, conceptual form of identification. In the simple form in which distance and extent occur within the data of any one sense, it cannot be said to vary truly as a modification. True variation may, however, occur within narrow limits, or by the integration of data of heterogeneous senses, *e.g.* touch and articular sense, to a large degree.

The attempt has been made to express the results of the measurement of sensations in Weber's law in terms of sensory distance instead of in terms of component units of sensation. But the least recognition of the nature of the modification of distance, as discussed above, shows that it has nothing to do with the conceptual or numerary order of just perceptible differences of any kind, even of distance itself. Distance is not integrated from any other attribute than order (*cp.* above, § 5, p. 153).

*Distance and the attention.* We have discussed the relation of the modification of motion to the attention, and have suggested that the attention is apparently attracted by motion, because motion in an otherwise resting field forms a single one of a class of experiences not represented and therefore seems to attract the attention as does a single light, a single sound or any other unique experience. Now it can hardly be said that distance exerts a strong attraction upon the attention. There is no doubt about its presence in the case of distance as given by the aesthesiometer, by separate points in an empty visual field, or as an accompaniment to any motion. In the last case its presence is as evident as is that of motion. In the first two cases and more especially when the two points are rather far apart and are not the only points excited, its presence is not so evident and unmistakable. For in this case not merely one, but all our visual experiences are modified by distance. There is therefore just as much rivalry in reference to the attention as when any group of similar experiences is given. So long as our sensory experiences are taken collectively or the attention is in any way helped to grasp a number of points as a unity, the integration of distance will be complete and exhaustive. We are all familiar with the effect of symmetry and balance of distances in this respect. If very many points are given and if the attention for any reason is directed closely towards one point, *e.g.* by its motion, there may be an imperfect psychical realisation of its distance. We very often notice a tendency to emphasise and heighten the effect of distance



by the conversion of a simultaneous distance into a successive one, as for instance, when we more accurately measure the distance between two points by looking from the one to the other. In this way one distance is separated from others by means of motion and reduced to the form in which sensitivity, at least to the threshold sensation, is greatest. It need hardly be added that distance is not realisable apart from its constituent sensations, whether actual or revived.

*Interval.* In sound distance appears as interval. The characteristics of the modification of distance are found in that of interval. Interval results from the integration of either successive or simultaneous tones. It is directly experienced and is not the result of judgment or of the conceptual comparison of the pitches given. It presupposes no knowledge or realisation of the absolute pitch, but only the psychical presence of tones of different pitch. Appreciation and comparison of interval can therefore occur in a perfect form without "absolute ear," as for example in the case of Wagner. Interval has no limits in respect of the pitch-differences of the constituent tones, although its successive form is limited by time-interval. Melody, as we saw, has both "space" and time-limits. Interval may occur without melody, but it is more urgent and clearer with it than without it. In fact many people can recognise interval only in its successive form.

The threshold of interval is peculiarly affected by the physiological peculiarities of hearing, which give rise to beats and intertones when tones of neighbouring pitch are given simultaneously. The difference between simultaneous and successive intervals is therefore marked. Small successive intervals are not disturbed by physiological excrescences as are small simultaneous intervals. But for these disturbances we might expect to find that the threshold for the simultaneous form is higher than that for the successive form<sup>1</sup>. For if the physiological theory of cortical representation used to explain touch-discrimination be adopted here, we should have a fusion of excitations corresponding to order-differences and with it a fusion of differences of voluminosity, which are much rougher. The result would be a rather more intense tone of voluminosity equal to that of the greater of the two tones and of slightly

<sup>1</sup> Cp. the facts detailed by Stumpf (25, Vol. II. p. 397). A tone, under certain circumstances, seems to be slightly lowered in pitch when another, considerably deeper, is sounded, and to be raised slightly, when a much higher one is given. This probably has a physiological foundation, as well as the psychological one that is exemplified in some of the visual illusions.

indefinite pitch. For all we know, this may be actually realised<sup>1</sup> in those cases in which the pitch of the two ears is different. But it cannot become a prominent peculiarity of the discrimination of tones. As in other forms, the distinction of direction in melody and interval has a higher threshold than has that of motion. Interval shows the same relation to the attention as does distance.

The appreciation of interval, as of melody, is independent of consonance, tonality and rhythm. It arises, as we must suppose, simultaneously with melody, and both are there as soon as the constituent differences of pitch are given. The origins of consonance, tonality and rhythm are quite separate problems. In talking of interval in the primitive sense, we cannot mean consonant, dissonant or "tonal" intervals. There can be no doubt that, whatever may be the actual state of human hearing now, interval is psychically conceivable and possible without any consonance, tonality or rhythm. It seems best to refer consonance to a physiological basis, whereby, owing to the partial identity of stimulation of a tone and its octave and the like, a partial fusion similar to that of simultaneous touches, too near to be distinguished or from neighbouring sides of two adjacent fingers, takes place. The recurrence and mutual compatibility of pitches seems to be quite another phenomenon<sup>2</sup>, which is known as tonality. For it a special explanation suitable to its peculiarities has to be sought.

## § 8. RETROSPECT.

These two, motion and distance, are the only modifications of sensory experience which result from the integration of the elementary sensations of one and the same sense. We have selected them for study in order to show clearly the peculiar modification of experience inherent in each, its derivation from a common attribute and the similarity of the phenomena peculiar to the same generic modifications, motion and distance, in the various senses. A number of other peculiarities of these modifications were mentioned and will be referred to again. For having thus established the general type of a derived modification, we shall now use our knowledge to classify certain experiences, hitherto supposed to

<sup>1</sup> Even without this, we must allow that the discrimination of simultaneous tones is not more wonderful than the discrimination of touches on the skin. In fact, our whole treatment shows that these processes are parallel. The arguments of page 143 are only special pleas. The extensities of sound cannot be supposed to overlap just because they are neighbouring or because one is greater than another.

<sup>2</sup> Cp. Stumpf (25, Vol. II. esp. p. 197).

be elements or aggregates, as modifications resulting from integrations as yet undiscovered. We shall thereby justify our starting point and by it advance to new knowledge.

There are many well-known modifications of sensory experience besides motion and distance. There is no need to attempt an exhaustive enumeration of them. Some, like the localisation of sounds and tonality, belong apparently to the products of a single sense. Others seem to result from the integration of sensations of different senses. Examples of these are depth and apparent size, the vertical direction in vision, and many complex forms of apparent motion and distance. Each of these will call for careful study. But that cannot be attempted here, for it is the purpose of this paper not to cover the whole field, but by a study of the simplest cases to draw attention to these new and highly important problems.

The phenomenological study of these other modifications of sensory experience presents no new difficulties. We can readily classify them in reference to the primitive attributes of sensation. We can explain their introspective barrenness and elusiveness, their attachment to sensation, their incapacity for isolated existence or recurrence, and so forth. Only the actual analysis of these modifications into their constituent elements, and the discovery of the whole mechanism of their integration, physiological and psychological, now present any difficulty. And that rests ultimately in our ignorance regarding essential facts involved in these complex integrations. But having succeeded in dealing with the phenomenological problems of our subject-matter, we may feel assured that we are on the path towards a solution of the new integrative problems which will arise.

#### § 9. CONCERNING THE SUFFICIENCY OF SENSATIONS AS ELEMENTS OF EXPERIENCE.

The efforts of the earlier psychologists of the associationist period seemed to lead to a clear conclusion. The only elements of mind appeared to be impressions or sensations and their counterparts in indirect revival, while the only bond of connexion between them was association. But although this result was eminently satisfactory and efficient in the first rush of study, on closer examination it was soon found to break down in many subtle cases. A subsidiary principle was therefore needed to account for the fact that the elements of mind do not always seem to survive in the complex state; for where no further



elements were forthcoming, psychologists were justified in seeking to explain, as well as they could, how the given elements could be thought to account for all known varieties of experience. Thus we get the conception of mental chemistry, which we can, of course, now easily recognise to be even in its origin mistaken. But at the time, reasoning by analogy suggested it as a likely manner of realistic interpretation of the mind. Such a conception breaks down, because we cannot apply those indirect tests that are pre-supposed by a realistic interpretation of the mind. Physiological tests of its correctness, even if they were unambiguous, are often practically beyond our reach; and we have had no success in indirect psychological tests, which might have proved the unconscious presence of sensory elements in states that could not be reduced directly to these elements. Whether these tests were ever actually carried out is a matter of indifference to our present interests. The chief objection to the view was based on its greatest difficulty. It did not explain how out of the elements given something arose which appeared to be essentially different from these elements. Only one explanation lay to hand—association; and as, in ordinary cases, no such radical change of appearance was produced by the action of association, its presence could not explain these mysterious transformations in certain cases. Instead of seeking an outlet by the postulation of new forms of connexion between the elements of mind, later psychologists allowed their minds to be impressed with the apparent qualitative difference between the elements and the alleged compound states. Thus we next find a growing conviction that at least feeling is an elementary state of mind, other than any of the known sensations. It might have to be classified as a peculiar kind of element with characteristics fewer or other than those of any sensation, but it must in any case stand apart. When this point was reached, the influence of the prevailing Kantian attitude towards knowledge and the needs of the experimental extension of psychology which had just come into vogue, checked any further advance for a number of years. Now that experimental observation has greatly extended the basis of psychology and a temporary exhaustion of the more obvious problems of the senses has encouraged the attack upon the less tangible states of mind, we find a rapid extension of this attitude towards feeling. Any mental state which is not clearly reducible to more elementary states is to be itself an elementary unit. So we find thoughts, conscious relations, attitudes, recognition and the like added to our lists of elements (6, 7, and others<sup>1</sup>).

<sup>1</sup> Cp. also 29: "Wiedererkennen ist als Bewusstseinsinhalt ebenso primär und unerklärbar, wie Rot oder Lust."

We have now to turn to the other side of the process and ask how the study of the modes of combination of elementary states of mind has progressed. Unfortunately, we find practically no advance whatsoever. The experimental investigation of memory and reaction has worked so successfully with the notion of association, that, in spite of all sorts of restrictions and parentheses applied to any suggestion of its universality, no other form of combination has been sought. Even the earlier attempts to vary the form of association by adding to mere contiguity the bond of similarity, contrast and the like, have been very often abandoned by experimental research. Whatever may be our final conclusion regarding association, it is clear that even in its primitive form it has been a most useful conception. Whether its statement is complete and adequate is another question, which need not be touched upon here. The only really satisfactory chapters of psychology of the present time deal with association. But the scope of this force is rapidly being traversed and its limits will soon become rudely apparent.

Psychology can hardly remain satisfied with such elements as thoughts, relations, recognition and feeling. All sorts of difficulties have already been raised regarding the last of these. What are its adequate conditions? What are the organs which subserve it? Why is it individually so very variable? It is not at all easy to construct a physiological theory to answer these questions. Much more must this hold for thought and the like. From the psychological side also many questions demand an answer. We want to know what characteristics these new elements have, so that we may be able to distinguish them as experiences from our sensational elements. And if their characteristics are other, fewer or more than those of sensations, we have to ask how they contrive to exist without attributes which are generally considered to be essential to the existence of sensations. A satisfactory answer to these questions will not be readily forthcoming.

Amidst the ruins of the old associationist theory in its various forms two parts remain intact and firm: the elements of sensation and the bond of association. We have seen how the distinction of new elements attempts to fill out the deficiencies and raise a new scheme of mind. But it is possible that the elements of sensation are, after all, sufficient in themselves and that it is our binding material that is insufficient and unstable. Considering the difficulties involved in the postulation of elements other than those of sensation, it is surely the more correct method to see how far we can carry our elements of sensation by the postulation or demonstration of a variety of forms of combination.

Only when we fail to progress on these lines need we recur to the differentiation of new elements. Their justification, in any case, will not be easy.

### § 10. FEELING.

*As sensation.* It is certain that feeling is a peculiar modification of experience, extremely unlike sensation. To try to reduce feeling to aggregations of organic, or, more especially, visceral sensations is a hopeless task (4). For, however decisively it may be shown that feelings are always accompanied by or are dependent for their occurrence upon some or certain sensations, no means has yet been established of proving that feelings consist of sensations. Feelings do not appear to introspection to be composite; and they do not show those sensational characteristics which we should expect to find in aggregates of sensations. Any decisive differentiation between feeling and sensation, therefore, precludes the theory that feelings are aggregates of sensations. For no matter how many accompanying sensations are tabulated, the feeling itself will always constitute an irreducible remainder. It need hardly be added that other peculiarities of feeling, especially its inherent reference to all kinds of processes, whether they be sensational, intellectual or conative, are not adequately explained by this theory.

On the other hand, the mere classification of feeling as sensation (26) is undoubtedly a weaker method of dealing with the problem. It is hardly possible, if a strict psychological definition (28) of each is sought. Only if we emphasise the discrepancy between different kinds of sensation, so that we treat them not as a type, but as a heterogeneous collection, can we sufficiently apologise for the inclusion of feeling amongst them. But to do so is to discount the value of what we thereby gain. If the value of classifying feeling with sensation does not lie in the introspective identification achieved, it must be found in the consequences for physiological and genetic theory. For the former a parallelism of relation between sensation and feeling on the one hand, and their sense organs on the other, is the weightiest proof. But here the difficulties are greater still. For an independent feeling, isolated from all reference to other experiences, must be of the rarest occurrence<sup>1</sup>. Any attempt to determine the sense-organ of a feeling-

<sup>1</sup> The occasional independence of feeling is witnessed by Külpe (14, pp. 227 f.); Ladd, whom Titchener quotes (28, p. 42), retains the reference of feeling, but denies any necessary time-relation of feeling to "the sensations and ideas by which we classify them." The



sensation is idle speculation, while various peculiarities of feeling to be mentioned later remain unexplained. Such a theory of feeling would be useless, even if it were possible. It makes no positive contribution to the explanation of any of its peculiarities. If it be said that the theory explains the rapid evolution of such an art as music, in which things formerly unpleasant are now very pleasant, it may be pointed out that, on the basis of analogy, more could be said against the rapid evolutionary adaptation of a sense-organ than for it.

There remain, therefore, only two psychological theories of feeling for our consideration. None other seems possible. Both maintain the unique peculiarity of the experience; but, while the one considers feeling as an irreducible element of experience, the other holds that feeling is the result of the integration of other experiences. To the former most psychologists of the present day adhere, while the latter has been advocated by Herbart and Lipps.

*As element.* Objections have already been raised to the view that elements exist heterogeneous to the sensational type. If the occurrence of extensity in some sensations makes it hard to admit its total absence in other sensations, we must find the case of feeling equally embarrassing. In dealing with sensation, we had the advantage of starting from a psychophysical definition which definitely grouped our material for us before we attempted psychological definition. Feeling was not included therein. For not only is its sense-organ purely hypothetical, but it has as experience none of that local precision and dependence upon stimulation which is sure evidence of dependence upon a sense-organ. Even if we could let that deficiency pass, we can hardly turn to a study of the compound experiences into which feeling enters in the hope of discovering thereby any latent attributes, not observable by introspection. There is no integrative modification of feeling to be thought of, unless it be the reference of feeling to other experiences, which it thereby qualifies. But that would probably necessitate the postulation of an attribute of order inherent in feeling, a clear, definite localisation or basis of psychical arrangement in the independent, isolated feelings. Since feeling can be excited by practically any kind of experience, we should then be able to arrange and realise a whole system of feelings, a feeling-world similar to our visual world, or a feeling-world which would really

extreme position held by Külpe is now modified to refer only to Gemeingefühle (v. 16, p. 185): "Die Einzelgefühle sind an bestimmte Einzelinhalte (Empfindungen, Vorstellungen, Gedanken und deren Komplexionen) gebundene Gefühle. Die Gemeingefühle sind umfassende, allgemeine, das ganze Bewusstsein färbende Gefühlszustände."

constitute a psychical universe. But, as a matter of fact, it is feeling which is placed by reference to other experiences. These do not constitute two separate systems, mutually coordinated like vision and touch.

*Integrative theories of feeling.* These have taken various forms. For Herbart feeling is the relation to one another of ideas which support or inhibit one another. Much the same is maintained by Lipps, with the addition of a direct reference to the relation of ideas to the ego. A recognition of this feature of feeling is also given in the earlier statements of Plotinus, Descartes, Leibniz and Wolff, that it portrays the momentary perfection or imperfection of the soul. We may neglect, for the moment, their use of the word knowledge, which for the sake of systematic statement we must here read as awareness, for there can be no suggestion that feeling is a state of knowledge. If we leave out of account the old superstitious craving for mystical unity in the greatest things, which led to the connexion of unity with perfection and therefore to the assertion of the unity of the soul, in spite of its many-coloured experiences, we may claim the view for this class. For the perfection of the soul was doubtless based upon the harmony of the soul and its experiences or of these amongst themselves. Why should the state of perfection of the soul otherwise change? Lastly, we find a similar view in the reference of feeling to the form of reaction of apperception to sensations (Wundt), or perhaps in the classification of feeling as a "*Gestaltqualität*" or formal, qualitative modification of experience. But none of these theories has explained why these relations should emerge as feeling and why feeling should have its many peculiarities. It is unnecessary to discuss the validity of these views now. It will be sufficient to point out, after the development of our own theory, in what their validity consists.

*Varieties and characteristics.* It is now commonly recognised that there are only two kinds of feeling—pleasure and displeasure. By some, *e.g.* Wundt, a multiplicity of qualities is advocated. The position we take up does not, however, require a preliminary discussion of this question. No better statement of the arguments against a multiplicity of qualities could be given than that of Külpe (16). These are: (1) the general comparability of pleasures and displeasures in reference to one another, whereby a methodical view of the value of experience can be obtained, no matter what its underlying qualitative differences may be; (2) the possibility of an unlimited interchange of feelings; (3) the indifference of feelings in reference to comparisons of sensations, images or concepts, whereby

a purely unbiassed, objective comparison of these things is rendered possible; (4) the fact of a general transference or irradiation of feeling, whereby a feeling dependent upon an experience (*a*) can be transferred to an experience (*b*), if there be a regular bond between (*a*) and (*b*); (5) the fact of a very extended analogy among feelings and the resultant possibility of a replacement of one impression by another or of the characteristics of one by another, whereby we can talk of a bitter sorrow, a sweet happiness, a tender regret, a rude misfortune, a cool feeling, an ardent sympathy; (6) the absence of direct influence of feelings upon memory and (7) the improbability of a great variety of pleasures and displeasures; for if we had this, it would be easy to arrange feelings on their own merits into a vast scheme, whereas, as we have already suggested, there is no such vast variety, but only the merest distinction of pleasant and unpleasant feelings in independence of the objects or states they qualify. As Külpe says, there is no need for feelings to express over again the variety sufficiently expressed by impressions, but only to show their nature, attractive or otherwise. This whole statement is of the greatest interest to our position, for, as we have maintained that the properties of sensation can be determined, not merely by direct inspection of them, but also by examination of their modes of combination with other experiences, so it shows that a broad survey of the forms of connexion of feeling with other states will help to settle the nature and forms of variation of feeling, even when introspection may leave these still in dispute.

These two kinds of feeling—pleasure and displeasure—(1) do not depend for their occurrence upon the stimulation of any one particular kind of sense-organ. It is remarkable that they (2) seem to leave no image, (3) are not reproducible, and (4) are not associated with one another or with images. They are (5) also very frequently, if not always, consciously referred to or attached to other experiences<sup>1</sup>. Feelings are (6) amenable to introspection only to a limited degree. Anything that tends to weaken or dispel the experiences upon which they are based, thereby weakens or dispels them. Feelings (7) vary in intensity, but do not seem to have any extensity or localisation, except in so far as they are attached to experiences which are localised and extended. Finally it may be maintained that feelings (8) arise only when two or more experiences are given, or that no single elementary sensation is of itself capable of evoking feeling necessarily or regularly,

<sup>1</sup> Cp. on these points, 16, pp. 183 f.



but that, if the feeling seems to be aroused regularly by some elementary sensations, as *e.g.* by those of the lower senses, taste, smell and the like, the regularity of occurrence is not absolute and is therefore dependent upon some other element of experience which is usually present, but may be absent. Whatever detailed casuistic may be brought against these statements, they have all a large amount of probability and may therefore be presumed in favour of any position which can use them.

*Comparison of feeling and motion.* Our theory of integration demands that we refer a modification of experience such as feeling to an experiential basis in more primary experiences and make a statement of identity between these two which shall be self-evident. We have every right to seek our primary basis of feeling in the experiences to which it refers or is attached. We saw in the case of sensory integrations how an integrated modification of experience is attached to its primary basis. Motion, *e.g.*, is inseparable from a sensory basis; it (1)<sup>1</sup> need not always be attached to one particular sense, but it cannot be experienced in isolation from all sensory forms. Even the recollection of a motion never implies the isolation of the experience from its sensory basis; for to dream of a movement is to dream of the successive sensations progressively different in order and so to realise afresh the experience of motion. In other words, (2) motion of itself leaves no image behind. This does not, of course, mean that we rarely think of motion that is not given by stimulation. Such a statement would be just as absurd as the declaration that we never think of feelings unless they are actually elicited, whether for the first time or afresh by the recall of the experience liked. We can think of motion or of depth<sup>2</sup> or of any experience we may have had, whether we can have it now again or not. But we usually recall events in single successive stages of projection and motion (3) is not reproducible in isolation by itself, but is re-created afresh in our experience when our memory of successive phases is sufficient to re-establish it. There is no evidence for the existence of a memory image of motion which differs from sensational motion, as the imagery of the usual sensations differs from these. The same is true of feeling. But as our theory provides an adequate basis for the re-creation of motion, so also may an integrative theory do for

<sup>1</sup> The numbers in this paragraph refer to those of the previous one.

<sup>2</sup> It has been said that we never recall depth in representation. Probably we seldom do so. The most vivid memory of depth I have noticed occurred when I was engaged in a special study of the experience of depth. I dreamt I saw a picture of a bunch of flowers in perfect depth-effect. In my dream I shut one eye to observe disparity of images and the depth-effect immediately vanished.

feeling. In the same way it would be easy to show (4) that motions do not associate with one another. So one process of recognition does not recall another. Not only is motion psychically inseparable from, but it is always (5) attached to, or more or less embodied in, the experiences which form its sensory basis. It is obvious, therefore, that (6) motion cannot as an experience be studied in isolation from its sensory basis. It is even impossible to lay hold of motion by itself and describe it. Motion is indescribable except in terms of the sensations upon which it is based. Of course, sensory data may be steadily maintained by the action of external influences, and we may exert our introspective attention to the utmost without disturbing the experience of motion, so long as the effort does nothing to dispel these sensory data. Motion is a purely mechanical sensory integration. Feeling, on the other hand, even when its primary basis consists of elementary sensory data, *e.g.* those of vision and hearing, seems to involve attention or some vague attitude in a subtle way. This accounts, however, not for the elusiveness of the pleasure experience for introspection, but for the speedy collapse of the sensory basis of a feeling as soon as attention is directed to other experiences than that sensory basis itself. Hence the rapid disappearance of feeling when introspection is turned upon it. We can bring the observation of motion into the same state, if we try to observe it in isolation from its sensory basis by diverting the eyes from the moving thing. This argument is, of course, not at all prejudiced by the fact that we have experiences of motion which, in the view of some, are really primary and irreducible, *e.g.* those of articular and labyrinthine origin. On the contrary, we must conclude that the very difficulty of these experiences for introspection is due to the fact that they are modifications of motion, which are not usually correctly analysed and whose primary elements are very weak when isolated, or resist isolation altogether. It is clear, finally (7), that a derived state like motion need not share all the characteristics of the sensations upon which it is based. Indeed it cannot. It is a secondary form of that attribute whose differences it integrates to unity, and it may show forms of variation owing to the influence of factors which affect the primary attribute integrated, as does motion in speed, in so far as the rate of change of order in time varies. A motion cannot, as such, be intense, or spread out or saturated. Feelings are of two "qualities," pleasure and displeasure, and they also vary in intensity. But they are not extended, localised, ordered, or saturated.

There is therefore a very close resemblance between the two,

experience of feeling and of motion, which would certainly justify their classification together. We have every reason to expect that the same kind of explanation is valid for the two states; and as we have found the theory of integration adequate to explain all the characteristics of motion, we may apply the same principle of explanation to feeling with much hope of success. However different feeling may be, as experience, from any others, it is clear, at least, that its characteristics are not unique and that it is probably the product of integration.

*Is the integrative basis for feeling sufficient?* Although in the previous pages we have shown the similarity between feeling and other products of integration, we have not yet verified for feeling one of the essential conditions of integration, viz. (8) the presence of a multiplicity of primary experiences in every case of feeling. There can be no question, however, that such a plurality of data is present in the vast majority of cases. It is the harmony of experiences, of colours, sounds, tastes, smells, motions, distances, objects and thoughts which is the object of feeling, *par excellence*. In many cases a single one of these is distinctly pleasing only because it is in some way a change from some other. The only formidable exceptions are found in the so-called lower senses of taste and smell. There we seem to find single, isolated, elementary sensations which evoke very pronounced feelings. All children and most adults find sweet tastes pleasant and sour or bitter ones unpleasant. At least there seem to be clear cases in which a merely sweet taste, as such, is liked, while strong, bitter tastes are disliked. In considering cases like these, we have to remember that the same sweet or bitter taste does not always evoke the same feeling, although it seems to act solely by itself. To the one person it may be pleasant, and to another or to the same person at another time it may be indifferent or unpleasant. In the face of the apparent isolation of any taste and its feeling, this has often been expressed by saying that the feeling evoked by a single experience is not due to psychical, but to physiological necessity, which again is to be referred to physical and chemical conditions or to the vagaries of biological selection. But such an explanation has already been shown to be untenable. For it either implies the existence of a sense-organ for pleasure or it denies altogether, or rather ignores, the possibility of a psychical causation, and it fails in any case to explain the peculiarities of feeling. We can really do nothing at all with the assumption that our experiences are merely hitched to one another, we know not how. Unless we can show convincingly that they are pure and primary datum, we must at least



endeavour to show some form of systematic and regular connexion between them. In so far as they are primary datum, we must be able to show that our experiences are immediately and regularly dependent upon some form of objective condition which is not experience, as we know it. In so far as they are not mere, primary datum, we must endeavour in principle to show that they are wholly and solely the result of the interaction of those experiences which are mere primary datum. If a primary element of experience is found in apparent isolation with what is obviously a derived modification of experience and then again is found without it, we may, on our hypothesis, fairly look for some undetected variable element of experience, present in the first case and absent in the second.

Fortunately there is evidence to show that some such variable element exists. An extreme case like that described by d'Allonnes (4) shows that the integration of feeling is impossible without internal visceral sensations. There is therefore no difficulty in assenting to the statement that a multiplicity of sensory data must always be present with feeling. In the case referred to, no distinction of feeling was made between a glass of water and a glass of castor oil; a choice was made in favour of the water only by help of conceptual remembrance. It would certainly be wrong to maintain upon the basis of this and similar cases that feeling consists of visceral sensations. For we should again fail completely to explain the peculiarities of feeling. If feeling were an integration or aggregation of such sensations, we should experience it as such, it should bear all the characteristics of sensation as such or of an integration of sensation, and it should be referred or attached to visceral sensation. In the case of the simple feelings, however, we find an attachment or reference, not to visceral sensation, but to all or any kinds of sensations or experiences which evoke them. That the visceral factor is not a direct constituent of the feeling itself is shown by the considerable unlikeliness and unexpectedness of the existence of a visceral factor at all. Feeling, therefore, does not consist of visceral sensations, nor are these the only essential element in feeling. If the parallelism of feeling and motion is of any value, it shows that one, at least, of the essential elements of an integration is that to which the modification which results refers or is attached. The pleasant sight or sound, the nasty taste or smell must each contribute one of the differing elements which constitute the integration. In the case of tastes and smells the visceral factor indicated by these abnormal subjects may be highly probable and acceptable as the integrative complement to the

exteroceptive sensation. For the sensations of the lower senses bear a clear reference to the internal, digestive apparatus. Our appetite is stimulated by them, they suggest inhalation or embodiment or they at least draw us nearer. But it is hardly so with sights or sounds. Lovely pictures and music do not often consciously stir our bowels or rouse our bodily appetites ; nor does their unpleasantness bring to our minds the dispeace of our organs. The pleasantness of pictorial art or of music seems usually to reside wholly in itself. It is pleasing or ugly solely on its own merits, or at least largely so, and hardly at all because of its effect upon the viscera of the connoisseur. At the most we say we do not care for a work, because it does not appeal to us, does not arouse in us, perhaps, the emotions and sentiments to which we are most prone. These latter experiences may be dependent upon and may carry a conscious reference to the internal organs of the body. But the peripheral, primary pleasantness of the sensations of the higher senses can hardly be thought to do so. This view is supported by the fact that it is so rare to find a single sensation of these senses which is pleasing, purely by itself. But there is, of course, nothing to prevent visceral sensations from being aroused by and integrating with these higher sensations upon occasion.

*Of what attribute of experience is feeling the integration?* Far from being peculiar to visceral sensation, it must be one which is universal in experience. We know that two or more different qualities of almost any group of sensations may form a pleasant or unpleasant combination, although differing only in order or place in time. We shall, of course, look for examples of this in those senses in which we do find a variety of qualities, *i.e.* in vision, sound, smell and taste. Senses like touch or those of the joints or muscles, which have little or no qualitative variations, seem to be more or less indifferent. Even a variety of orders, as in visual, pictorial arrangements, of durations, as in rhythm, and possibly also of intensities and extensities, as in the arts of vision and sound, may be pleasant or unpleasant, without any other accompanying differences. We do not usually find that qualities of different senses combine to give pleasure or displeasure, unless we except those single sensations of the lower senses of cold, warmth, pain, taste and smell in their conjunction with sensations of visceral origin. Differences in a secondary modification of experience are also often the object of feeling. A complex of motions, of distances or of depths may be liked or disliked, while this can hardly be asserted of any single one of these. In view of this fact and of the high frequency of feeling in reference to

the senses of vision, sound, smell and taste, which already show direct or indirect evidence of integration, it might be thought that feeling is the index or result of the mutual harmony of integrations. It is certainly not a regular accompaniment of integration as such. It would have no *raison d'être* and could not explain itself as such. If feeling means the harmony or smooth working of the mind, this can only commence with a second level of integration, if at all. It would be rash to attempt to go beyond these conclusions at the present time to a specific theory of the attributive basis of the modification of pleasure. The results of experimental investigation are too scanty and contradictory to give any clear leading (8). We need not be surprised that we are meanwhile unable to point to the integrative basis of feeling, although we have made the demand that psychological explanations must be causal and self-convincing. In the articular and labyrinthine "sensations" of movement, we have examples of experiences the reduction of which baffles many psychologists, although the integrative basis is in these cases moderately obvious. We shall hope to be able to explain feeling completely, when our attention has been drawn experimentally or by analysis to its attributive basis. For this purpose, we must have more details of an introspective nature concerning the moment of realisation of feeling. There need be no particular difficulty in applying introspection fully, for we do not need to introspect feeling itself, but only its sensory or other accompaniments. We have the further guidance of the variation of intensity peculiar to feeling, which, after the analogy of motion, would suggest that the attribute of which feeling is the integration is capable of a variation by degree, similar to that of intensity in the sensations. The duplicity of quality of feeling also suggests that we have to deal here, not with a simple, primary attribute, but with an integrative activity of some kind, which is capable of reversal. The activity theories of feeling seem of all to be nearest the mark. It is quite unnecessary to point out what theories of feeling of a metaphysical or other nature are completely discounted by the integrative theory here advanced.

### § 11. RECOGNITION.

It is generally acknowledged that recognition is a peculiar experience which calls for some explanation from any theory of the constitution of mind. It has hardly been claimed as a sensation; the prevailing tendency has been to treat it as a complex experience or as a unique, non-sensational element.



*As sensation.* On sensationalistic principles, recognition is easily accounted for. It consists simply in the revival of those sensations which were previously given simultaneously with the complex of sensations now recognised. On general principles, it is explained in the same way, without the restriction to sensation. Experimentally this has been verified in so far as it has been shown that such revival does accompany or follow recognition in the vast majority of cases (9). Cases of revival are, however, possible without accompaniment of recognition. We seem unable to say what kind of recall constitutes recognition. Although experimental analysis gives an almost general rule, the synthetic statement of it seems very unsatisfactory. Put a number of actual and of revived sensations together and does recognition supervene? Surely not! We miss some proof that the elements or experiences given are actually such as can be shown convincingly to give the state to which they are equated. The difficulty of such a synthesis is only increased when we find that the state of recognition can supervene before the sensations upon which it might be based are revived at all<sup>1</sup>. An explanation of this is sometimes attempted by supposing that recognition can occur when associated experiences are merely excited and not yet actually revived; as if a tendency to reproduction, in some physiological or real psychical sense, could produce an effect upon the experience which is to be recognised, without reviving in experience the states which it serves to reproduce. For the present, we refuse to discuss a theory which thus begs the question and does nothing to explain the psychical peculiarities of the state of recognition. It can be sufficient only where everything else fails, where, as in the case of the sensational elements of experience, there is left nothing psychical by the use of which we might attempt to explain their peculiarities. But it is not claimed in this case that recognition is an isolated, elementary state; for it is firmly attached to the state recognised and does not occur alone. These and other peculiarities call for some explanation, which such a view cannot give. For, while its theory may be sufficient physiologically, it is insufficient psychologically, in that it cannot explain how the state of recognition comes to be hitched to one out of, possibly, many experiences of the same kind. There can be no doubt that the state of recognition is, at least, a modification of experience which is not identical with any one

<sup>1</sup> Cp. 1, "Dans l'acte de la reconnaissance le souvenir se joint à l'impression avant qu'il se développe en image."

sensational element or mere aggregate of these on the lines of associative synthesis.

*As element.* But it has been claimed that recognition is a peculiar non-sensational element of experience. Against this we have to urge, as before, that nothing has been done and probably nothing can be done to explain the psychical peculiarities of these elements. The acceptance of such a view bars the possibility of any closed science of experience, at all complete and self-coherent. We are left with a psychical pluralism which does not even invite reduction. It must be said, however, that the acceptance of non-sensational elements of experience is not really an independent view, but is only an expression of the recognition that certain states of mind are unique in character and very unlike sensation and have, so far, defied any satisfactory reduction to sensation.

It may be noticed that recognition does not seem to occur entirely in isolation. I am not aware that the contrary has ever been maintained in this case, as has been done for feeling by Ladd. It would seem absurd to think that recognition should occur as a state without any experiences which might constitute an object for it. It may seem equally absurd to make a similar statement for feeling, unless for a general state of feeling, in distinction to the special, detailed state not referred to the self as a whole. Except that it may be established that other experiences of some kind always do form one of the conditions of the appearance of recognition, there is no apparent reason why recognition, if it is an element, should not occur in isolation. The view which considers recognition elemental might, however, point to false recognition as a case of the partially isolated occurrence of the state, apart from its correct and realistic implications. But here, again, we find no explanation of the reference of recognition to an object, correct or false.

*As secondary modification of order.* We must hence revert to an integrative theory of recognition. Clearly this modification of experience bears a strong resemblance to the attribute of order, as we find it in the sensations, in the form of localisation or especially in sound as pitch and secondarily as localisation, or in the modifications of motion, distance, depth and perhaps direction. Recognition is "qualitatively" the assignment of a place or order of a special kind to an experience which, so far as its elements, their attributes and integrations are concerned, is in recurrence<sup>1</sup>. It may, therefore, be classed as a

<sup>1</sup> The notion of mental causality cannot demand a recurrence of the real material of mind, as if our experiences, once had by us, went off on a round by themselves and then

secondary modification of order. It seems, however, to be an unvaried modification and, in this respect, unlike motion and feeling. We either recognise or we do not. Our certainty and clearness of recognition vary somewhat, but these variations can hardly be said to be necessary and characteristic, as are those of feeling and motion. They are more probably based upon the process of recall involved in recognition than upon the integration which constitutes recognition. The falsity or correctness of recognition is also no true variation of it, but is rather another modification of experience which may occur in many other connexions than that of recognition. The order given to each experience by the recognition of it is, of course, different, as is the order of different sensations of the same quality, intensity, extensity and duration, but the recognition-order of one and the same experience does not vary. One and the same experience may be recognised by different contexts, but in this case a radical change has been made in the complex which integrates to recognition, similar to that which occurs when an object that has aroused pleasure in one mental setting, arouses displeasure in another mental setting. This is no true variation of recognition, but a change in the states revived and thereafter integrated to recognition. Its explanation, therefore, belongs to that of reproduction in general.

*Comparison with feeling and motion.* Recognition (1)<sup>1</sup> does not depend for its occurrence upon the stimulation of any one particular kind of sense-organ. It seems (2) to leave no image of itself behind. This does not, of course, mean that we never think or remember of having recognised anything that is not now presented for recognition. It means, that if the state of recognition is ever "revived," it is re-created afresh by the recall of the experience which was recognised, the subsequent recall of the context of recognition and the integration of these experiences anew to the unity of recognition. But if this occurs outside the efforts of introspective experiment, it must be of very rare occurrence. Obviously, then, recognition (3) is not reproducible in isolation, nor (4) do states of recognition associate with one another. Recognition is (5) always attached to or, more or less, embodied in the experiences which constitute its integrative basis and cannot, therefore, (6) be studied introspectively apart from that

as *esse ipsissima* returned. We cannot expect to go further than identity of quality and all attributes of each element. Anything more than this would lead us out of psychology into metaphysics.

<sup>1</sup> These numbers refer to those on pp. 187 and 188.



basis. It is of the most fleeting nature and vanishes as soon as the attention is turned exclusively upon it. It resists description except in conceptual terms referring to the experiences which form its integrative basis. Recognition, finally (7), need not share the characteristics of the experiences upon which it is based. It has and can have no intensity or extensity or localisation in space or the like. It shares all the peculiarities of an integrative modification of experience and has in special those peculiar to integrations of order<sup>1</sup>.

*Is the integrative basis sufficient?* An integrative theory of recognition will demand a reduction of the state to the integration of differences in order of the states upon which it is based. We must, therefore, be able to show (8) the presence of a multiplicity of more simple experiences in every case of recognition. But it may be pointed out that such a multiplicity cannot exist in every case, for the state of recognition has been admitted to occur before the revival of the circumstances of the first occurrence or without the recall of the first experience, in so far as it was identical with the data now recognised. We here face an important problem which can only be settled by a comparative study of the integrations of sensations of different senses, such as constitute our full and complex space-perception. For the present, we suggest that an integration seems possible that leaves the quality and other attributes of the objectively less interesting elements very much in the background, while making use of their aspect of order for the purposes of integration. A lengthy search may be necessary to reveal the presence of the elements to which one of the differing orders belong. For unless the orders integrated are very little different from one another, why should we expect the qualities, extensities or intensities, whose orders are integrated, to be near one another in the focus of attention? Even in the case of next neighbouring orders, there is no sufficient reason why we should expect this, except in the case of the primary aspect of order, attributive to the

<sup>1</sup> It is an obvious conclusion from the whole work of this paper, that states like feeling and recognition can be attended to just as well as can sensory states. We should find no great difficulty in stating the attributive relationships of any secondary modifications of experience. But attention cannot find in any mental state what is not there to find; and we can have no desire to make in reference to it a needless assertion of incapability. The argument of the text, especially that of point (6), is, therefore, justified only by reference to the usual similar statements made for feeling. Recognition disappears as soon as the attention is turned upon it exclusively, because to do so is to divert the attention from the integrative basis of recognition and to destroy it.

elementary sensation<sup>1</sup>. It is just the aspect of order whose integration takes place in this particular case, and not that of quality or intensity. It is, of course, impossible to specify *a priori* the time that may elapse between the occurrence of a state with recognition and the presence of the revived state. It would even be difficult to show that the latter is not present simultaneously with recognition and that its apparent absence is not due merely to the fact that it has not yet passed before the objective of introspection. But it would be unworthy to base a position upon such a possibility. We know from experiments on abstraction (15), that the orders of certain elements may be present and admitted introspectively without their qualities being distinctly introspectable, even if present; and we know that qualities can be given and distinguished without their localisation, right or left of one another, being introspectively distinguishable. The possibility of the separation of the attributes for introspection, even if only for a moment, must be admitted. Where, therefore, integration refers to one aspect among several and unites it to a similar aspect in another state, round which as a unit the interest of the moment concentrates, there we may expect a still greater separation of the remaining aspects for the introspection. We may maintain that recognition is based upon the psychical integration of the order-aspects of percepts, although it is often present before any associated percepts can be identified introspectively.

The integration of recognition and full revival may properly be considered to be different processes<sup>2</sup>. It is one thing for a state to be revived and joined to another to form an integration, and another thing for that revived state to be considered by itself and identified. We should expect with as little reason always to be able to state the direction or to distinguish from one another the constituent elements

<sup>1</sup> The focus of the attention and other similar terms are misleading, in so far as they suggest that our experiences make their way to the centre of a circle, as it were, dragging all connected experiences with them, more or less. On the contrary, it may be claimed that only the order-aspects of our experiences change without any movement of attention, unless we use that term to indicate the direction of the integrative and associative processes now taking place. If we recognise an object, therefore, our 'attention' must necessarily be directed to the process of recognition and the object recognised, but it need not be directed to the associated states which form the integrative basis of recognition, unless these take up the work of integration and recall, as they often do.

<sup>2</sup> Such a psychological account agrees well with the physiological theory which postulates the partial or incipient excitation of reproduction-tendencies to explain the occurrence of recognition without actual revival.

of a motion or distance, as soon as we appreciate the presence of these, as always to be able to refer a recognised state conceptually or otherwise to its first occurrence. We found this difficulty already in the case of feeling, where the apparent isolation of the integrated state with one side of its integrative basis is in some cases even more pronounced. Recognition, therefore, may be accompanied by the assurance of its correctness or by the knowledge of its point of reference in experience, or it may occur without these (20, pp. 39 ff.). Experimental investigation reveals the presence of a direct unmediated experience of recognition, which can occur alone and must be considered to be the only true form of recognition (11) in no way to be confused with the conceptual inference of "previous presence in experience" from certain "criteria" (20). It is impossible that recognition should always be based upon such criteria, formal or otherwise, as Meumann (*ibid.*) suggests, for there is in primitive experience no means whereby any formal or other characteristics of experience should be made to stand for previous presence, unless an experience which conveys the fact of recognition to the mind is first given. If this state is once given, all sorts of criteria or means of certainty may be found for it. This truth must be recognised once and for all, if psychology is to be freed from hitherto incessant fallacy of argument. Integrative processes which contain their full determination within themselves are absolutely necessary, if experience is to be explained; for experience is not the product of mere incoherent chances. Recognition and assurance are both integrative processes of a function and character different from the process of conceptual reference within experience. All three may take up different time-relations to one another.

*Recognition is not a modification of the time-aspect of experience.* To recognise may often mean to refer a present percept to a previous occasion, but it does not do so as a modification of the time-aspect. Recognition, as experienced, does not vary in the sense of "a little time" or "a long time." Although it may undoubtedly lead to a fixation in time, this time and the process of fixation are both conceptual, and not experienced modifications of the peculiar non-conceptual kind of recognition and feeling. Recognition conveys in immediate experience the fact that a given percept falls into a certain perceptual place or order. How that place is made explicit or conceptual, whether by circumstance of time, of name, or of thought, is quite immaterial in the present connexion.



It is important to notice, in the next place, that recognition presupposes the modification of order that is characteristic of the percept and its integration. It is unnecessary to go into the problem of the conceptual or general percept at the present. We may confine our attention to the particular percept, "that here and now." That the particular object of perception is a modification of order will hardly be put in question. Its full explanation presents peculiar difficulties, which, however, are not our present concern. Integration of the elements of experience to perceptual units must precede recognition. For the revival of order that might be evoked by the bare elements of experience, would be that of other forms of primitive, attributive place-order, and not that of secondary, recognitional order. The former, however, would give only some illusive increase of extensity or the like, and not recognition. If a bare element of experience is ever recognised, it must first become a percept and acquire the perceptual modification of order. The integration to the effect "that (tone, colour, object, face, word) here" must precede the integration to the effect "that here has been" or "that here is that there," both now, when recognition takes place, and then, when the experiences included in the recognition were first given.

Recognition implies simple revival, as we have seen. Once set up, revival may proceed, beyond the amount presupposed by recognition, along any lines open to it. Generally speaking, it will follow the lines of least resistance. In the case of the first recurrence of a percept which has not entered into any other integrative processes than perception, the freest line will be that of a revival of the circumstances of its first occurrence and it will therefore lead to recognition. But a percept which has been extensively manipulated in experience on the occasion of its first occurrence, may upon recurrence excite other lines of revival and may, therefore, be illuminated by the light of other modifications than that of recognition. The more these other lines of revival are strengthened, the more these other forms of integration are produced, the more should we expect to find that recognition recedes on the average, until it disappears entirely. This is confirmed by an introspective examination of the course of our experience. It would, of course, be absurd to extract from this statement the implication that I am, as a psychical actuality, unfamiliar with my most usual surroundings; for that would suggest that the statement made implies the presence of unfamiliarity in the absence of familiarity. There can be no such real implication. Really habitual surroundings are generally

modified in many ways other than that of recognition. The flush of familiarity is experienced not by the hearth-bound native, but by the returning wanderer, whose first concern is to recognise.

## § 12. CONCLUSION.

*The classification of experiences.* Our study of the two integrative modifications of experience, feeling and recognition, serves to demonstrate the method to be followed in the study of all those forms of experience which are not conclusively elementary. What is to be considered elementary will be decided in the first place by the standard of the sensational type. Forms of experience that show most or all of the characteristics of those modifications whose manner of integration has already been traced, will be considered to be the result of integration. But if they are not reducible to elements already catalogued, their integrative basis must be sought in hitherto undetected elements. Any states of mind which may remain thereafter may be elementary or integrated. Their exact classification will have to form the object of special inquiry.

Feeling and recognition represent for our study the two great fields of affective and intellectual states. Whatever limits may ultimately have to be set to our procedure, we have, at least, shown that it is applicable over a large range of experience. No effort has been made to conceal the difficulties which faced the complete explanation of feeling and recognition. On the contrary, the hope may be expressed that something has been done to make them more distinct and assailable. Several other difficult investigations will probably have to be made before the theory of feeling and recognition can be completed. For these reasons, it is, for the present, quite unnecessary to apply our method to forms of experience similar to recognition and feeling, however interesting the task would be. Having shown how the study of such experiences is to be approached, we may leave it to the reader to make further applications himself. If the value of our method has been appreciated, that will readily be done. Each integrative form of experience will call for special study. There can be little doubt that there are a vast number of these. Research has already begun to discriminate some of those which group themselves round the term "thought."

*The gain for the experimental study of thought.* In this connexion it is interesting to consider briefly what positive gain accrues to

psychological study from our method. Two items may be put to its account. The first concerns the experimental study of thinking. Although this has made much progress in the last ten years, it has, so far, been impossible to show conclusively upon what experiences certain special states of mind are founded. Thought itself, for example, without prejudice to any theory, may be called a peculiar form of experience, not obviously of the same type as sensation. It may, nevertheless, be held that it is reducible to sensation, and to this end all accompanying sensations will be carefully recorded. But, as we have already pointed out for feeling (§ 10), if any sort of discrimination is made between thought and sensation, it will always be impossible to reduce thought without a remainder to sensations or to other elements. Thought is either clearly an aggregate of sensations, in which case it will, at least, show all the characteristics of sensation; or there is obviously no such thing as thought, except in common parlance; or thought is a peculiar form of experience, irreducible on the basis of exhaustive experimental introspection alone. Our service to experimental research consists in showing how a connexion is to be traced between thought or any other peculiar experience and the distinguishable contents of the mind that accompany it. There is then some hope of mediation between an extreme sensationalism (28a), which either reduces thought to a mere name for groups of experiences or utterly fails to explain those characteristics of it that are not evident in elementary sensation, and an extreme elementism, which discards all sensational accompaniments as irrelevant and builds solely upon the unity and peculiarity of thought (6, 7). We combine these two views by recognising both the relevance of the accompanying experiences and the peculiarity and unity of the thought.

*The gain for genetic study.* In the second place, our theory makes a positive contribution to genetic psychology. In showing how any complex modification of experience is integrated out of simpler forms, we are able to delimit that particular state very much more carefully from others closely related to it. Knowing fully its adequate conditions and its nature, we shall have more success in determining at what point of development it can arise. There can be little doubt, for example, that a large number of animals have particular percepts and recognise them. They need not, however, necessarily be able to locate these percepts in their past experience conceptually or have the assurance of the correctness of their memories and the like. We are,



therefore, freed from much of the restraint that is put upon comparative psychology by the fear of imputing some form of conceptual thought to the animal mind.

On the whole, finally, it may seem probable that sensations are the only elements of experience and that all apparently different states of mind are modifications which result from the integration of these sensations in respect of some common attribute. But we put no special value upon this conjecture at present. For the moment we would only claim careful attention to the method we have followed. Our work may be incomplete at every point. We have pointed out that our enumeration of the attributes is imperfect. Our enumeration of the characteristics of integrated modifications of experience may need amplification. And we have only selected the two most obvious and easy examples of these for study. There may be dozens of others. It is only in order to characterise and name our theory over against sensationalism on the one hand, and elementism on the other, that the word "modalism" has been appended to the title of this paper.

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# THE RELATION OF MIND AND BODY. II.

BY

HENRY J. WATT.

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## THE RELATION OF MIND AND BODY<sup>1</sup>. II.

By HENRY J. WATT.

1. *The basis of the problem.*
2. *Presuppositions of the problem.*
3. *Philosophical references.*
4. *Correlation between the psychical and the physical.*
5. *The attributes of sensation.*
6. *Some difficulties urged against parallelism.*
7. *The problem of psychical analysis.*
8. *The laws of psychical fusion.*
9. *The visual systemic sign.*
10. *Sufficiency of positive statements.*
11. *Psychophysical correlation.*

1. *The basis of the problem.* The scientific problems concerning the connexion between mind and body rest ultimately upon knowledge common to us all. One of the first tasks set to the growing mind is that of distinguishing between fact and fancy, between the present and the remembered. In psychology this practical task reappears in the theoretical difficulty of distinguishing sensation by definition from its virtual copy, the mental image. Sensation and image seem to differ from one another in respect of no aspect or attribute. They may be of the same quality; a remembered rose may be as red as a seen one. Their colours may be equally intense, equally extensive, similarly located—identical in all respects. Very often we fail to make a distinction between sensation and image, as in dreams, in hallucinations, and in many of the common perceptual processes of seeing and hearing. But at other times we distinguish them easily, whether by their different behaviour or by their different relations to the will and to attention. Sensations seem to behave in their own way; we must follow their leading: images come and go as we will; they appear

<sup>1</sup> Read before Section I at the Dundee Meeting of the British Association for the Advancement of Science, 1912.

and change at the bidding of our thought and attention. However this may be, it soon becomes clear to the mind just expanding into a knowledge of the world and of itself that sensations imply the stimulation of the body by some present object, whereas imagery is an inner vision which the mind retains and recalls without the aid of the body, even though the object remembered is gone for ever.

But a more minute examination of the facts inevitably shows that not only the mind's images but all memory and even the most rational processes of thought are in some crude way dependent upon the integrity of the body. Certain parts of the brain are found to subserve the processes of seeing and hearing, and injury to them will make the memory of sight or sound impossible. From this knowledge we may rush to the conclusion that every mental operation, however minute and special, is dependent upon the operation and cooperation of some special mechanism in the brain. But doubts began to rise when we find how difficult it is to say precisely upon what kind of cerebral mechanism the processes of thinking and reasoning depend. The problem then calls for the most impartial and careful study, in which hypothesis and inference are restrained as much as possible and all extraneous argument is excluded.

2. *Presuppositions of the problem.* It is evident that even a temporary solution of the problem *presupposes some sort of satisfactory knowledge of each of the two correlated fields*—the physical and the psychical. In the physical realm this knowledge may be said to have been attained. There uniform and more or less consistent schemes of arrangement and interconnexion of elements and other units have been adopted. We have some general schematic understanding of the anatomical and physiological dispositions of the central nervous system. Although every particularisation of that knowledge is extremely difficult and arduous, it may be presumed that further research will hardly yield results so strange and so surprising that they will not harmonize readily enough with what is known. This must be fairly obvious, I think, in spite of the fact, that we have detailed knowledge only of certain of the simplest and most mechanical of nervous processes—the reflexes—and that apart from certain facts of localisation we know next to nothing about the nervous processes which subserve our most usual mental events.

But if the outlook on the physical side is free and unbounded, it is as yet hardly so on the psychical side. Various causes contribute to maintain the obscurity which prevails. Very extreme views have been

held regarding our ability to know about the mind. Some have supposed that the mind is an open book to be read—by its owner at least—without effort or study. How could it have its characteristics at all, he would ask, without his being aware of them? Others have supposed that, far from knowing all, they know and can know nothing about the mind. Knowledge of it could not be expressed in systematic, general, scientific form. How could sensations, feelings, thoughts and efforts possibly be treated as natural objects, to be described, analysed, and classified? Both of these views are really untenable. A middle way must be followed. We must suppose that we are aware of all that passes through the mind, but that we attend to, describe and name mental processes only relatively seldom and then usually without much regard for systematic values. That is, of course, perfectly natural; for we use our minds, as we use the things around us, at first perceptually and practically, and only long afterwards conceptually and theoretically. Our scientific difficulty is, in fact, just this—to learn how to bring about any desired mental process experimentally, how to attend to it, to describe it repeatedly and fully, and to set it into systematic relation to all other mental processes.

This is the more difficult to do, as we naturally tend to notice first only those mental states which are relatively complex, e.g. emotions, thoughts, and memories. These states must first be analysed into their simplest parts before they can be brought into systematic relation to one another. To many the very idea of such an analysis is repellent; they maintain that no analysis is possible; it seems to them to dissipate its own object. To these thinkers the results of analysis seem to be discrete, independent particles, incapable of re-uniting to form the thought, emotion, or memory, from which they were derived. But such a view seems to me to be but the natural result of a method of analysis which proceeds without any sufficient attempt to maintain a corresponding theory of synthesis and interconnexion. There are many difficulties peculiar to such a theory, difficulties which are not at all like those with which we are familiar in physical science. And we must guard against applying to the results of psychological analysis the notions we entertain regarding physical elements. Being unsuitable in many respects, these notions only serve to form a prejudice against psychological analysis. We must establish our notions of psychical elements on a purely psychological basis.

Various schemes for the systematization of experience have been propounded; but none has been definitely accepted. The science of



psychology is at present in the unenviable position of having no concentrative hypothesis which is accepted as a skeleton for all text-books and as the ultimate motive of all detailed work and theory. The theoretical advance of psychology must, therefore, lead to results which will transform and enormously extend the outlook of present theory and which will lead to some unity of opinion upon the general lines of theory and exposition. The need for such a theory speedily becomes evident when we approach questions of detail regarding psychophysical correlation.

3. *Philosophical references.* The specially *materialistic* treatment of the mind as a sort of subtle by-product of cerebral activity may be dismissed, as it rests upon a totally mistaken and ignorant view of experience. It may safely be maintained that if there is any realm where interdependence of parts and systematization seem to be patent, that realm is experience. We are apt to look upon experience as a succession of unrelated particles, determined only from the side of the body, because a bodily counterpart or governing influence is often found where we might least expect it, and because experiences seem to affect one another for change little enough, so long as we are not actually enjoying them. After periods of forgetfulness they return to us much in the same relations as we left them in. But this feature need not indicate a lack of interaction between them. The interdependence of experiences may be largely static in character. The sphere of mental dynamics seems to be the consciousness of the moment, where new elements appear and where potent forces continue for a while, using their powers to produce vast changes. How far abroad from the present moment the influence of these forces extends is not yet exactly known.

We may also well refuse to make any reference here to philosophical problems concerning *the nature of reality*. We must accept as valid the naive view that there do exist things that are not wholly to be identified with the momentary contents of the individual mind and are thus far independent thereof. We may also accept the common distinction of this class of things as 'material' or 'physical' from the class of experiences or 'psychical' things. For the problem of the reduction of this distinction does not affect the problem of body and mind. The problem of body and mind is, and will always, from a preliminary point of view, remain the problem of the connexion of these two classes of things—physical, psychical.

4. *Correlation between the psychical and the physical.* Whatever

the nature of the connexion between body and mind may ultimately appear to be, it must rest upon some form of correlation between physical and psychical things. Both psychophysical parallelism and interactionism involve such a correlation, so far as they agree that some or all psychical processes are accompanied by or are evoked by physical processes or *vice versa*.

No one, I think, would venture to suggest, on the basis of positive considerations, that every single aspect of all the physical processes of a single class, no matter how restricted it be, is represented by an exclusive psychical correlative. Chemical and physical knowledge has grown so minute and so dense as to form an effectual barrier to any such attempt. Attempts have indeed been made to correlate molecular complexity with psychical relationships. The occurrence of consciousness itself is sometimes said to be dependent upon the occurrence of molecules of enormous complexity<sup>1</sup>. Hering's theories of adaptation also make some vague reference to processes of assimilation and dissimilation. And certain aspects of colour theory, especially the independence of white-grey-black vision, complementary relations, the facts of colour-blindness and of the distribution of colour sensitivity<sup>2</sup>, sometimes elicit speculations regarding molecular relations. But these theories could hardly be said to offer any hope of doing justice to all the features of molecular constitution. The spatial arrangements of the relatively enormous units of the nervous system call, of course, for closer consideration; but even of these only the more central dispositions are usually brought into correlation with experience. The problem of correlation, if it is to be in any sense exhaustively treated, must be stated from a psychical basis.

*These various considerations make it clear that the first step towards statement and solution of the general psychophysical problem must be the formation of an exhaustive catalogue of psychical states of all kinds, properly classified as elements, compounds, or other kinds of derivatives. For every one of these and for every distinguishable aspect thereof we must ask whether some satisfactory correlative cannot be found among known or possible physiological processes and their predicable qualities. Unless this is done, I do not think that the problem of the connexion between body and mind can even be raised. Every setting of the problem presupposes at least a temporarily sufficient completion of that*

<sup>1</sup> W. McDougall, *Body and Mind*, 279.

<sup>2</sup> Cp. C. L. Franklin, *Mind*, N.S. 1893, II. 473 ff.

task. The greater our success, the more extensive will be the psychophysical correlation be; the limits of correlation will be given by repeated failure to find or to conceive for each mental state a physical correlative. We must take heed, however, lest the problem of correlation change under our hands to the effort to identify the physical and the psychical or to resolve them into one another. The temptation thus to transgress the bounds of the problem is very great. After finding marked traces of correlation between psychical units and physical units, we are apt to look for a correlation between psychical laws and relationships and physical ones, forgetting that the problem of correlation only exists because psychical units differ radically from physical units and yet at the same time seem so to differ from one another in their own specific ways that their differences can be correlated with the steps by which certain physical units differ from one another in their specific ways. Neither the units of either side nor their absolute differences can be compared with one another, but only their relative differences in so far as these constitute a series of a regular nature on either side. If only the regularity, and not the differences themselves, can be compared, neither can any other laws nor relations on either side be brought into parallel except in respect of formal characteristics, such as regularity, unity in difference, etc.

No more facile and useful scheme of psychophysics could be imagined than that of the associationist psychology, which in its essential form must always hold an important place in the field of psychological theory, however far that may ultimately extend beyond the bounds set by the primitive forms of associationism. In the latter the only important distinctions were those of quality and of simultaneity or succession among sensations and images. Quality could be correlated with the varying localisation of function, familiar to common knowledge and later confirmed by the facts underlying the law of the specific energy of the sensory nerves. Different qualities were not thought to be separated by distance or by anything else; they were simply fused by aggregation into a percept; hence the separation by distance of the areas of localisation created no discomfort. If it came to view at all, it did so naturally in the time intervals between successive images. These intervals were, of course, to be correlated with the time taken for excitation to travel from one sensory area to another.

Whatever prominence may now be given to the notion of association, we have already gone far beyond the simplicity of the early psychology of this school. We are familiar with an enormous amount of detail,



especially in the field of simple sensation, which was then unknown. But attention has been concentrated mainly upon the discrimination of absolute and differential limits and the registration of the corresponding values of stimulus; the problem of the systematization of the simple sensations has been comparatively neglected. It has been thought impossible and useless; impossible, because the varieties of sensations we find seem so disconnected psychologically that only the accidents of biological development could give them some sort of explanation; and useless, because many ways of classifying attributes seem possible and none seems to add anything to our knowledge of sensation in general. But it must be evident that the true understanding of the physiology of the senses can only come when we have succeeded in classifying sensations and their attributes properly, so as to make them throw light upon the general constitution of experience. Even before we reach this point, we should find that the true classification of attributes makes the physiology of the senses more coherent and acceptable. For the psychological facts are just the key to the arrangement and interpretation of the physiological facts.

5. *The attributes of sensation.* The most direct attributes of sensation are commonly known as quality, intensity, extensity, local sign, duration, and position in time. The presence of intensive differences in all kinds of sensations, with the possible exception of vision, is undisputed. Differences of opinion exist regarding the quality of auditory, kinæsthetic, labyrinthine, and muscular sensations and regarding the extensity of many sensations, especially the auditory (where some recognise it in the form of voluminosity), and the olfactory, kinæsthetic, and muscular (where it seems to be absent). Nativistic and genetic views of the nature of local signature oppose each other and call for different physiological theories. The attributes of duration and position in time are at present perhaps completely in question. They seem at once the most obvious and the most obscure of all. The attribute of duration finds most general acceptance, which is the readier as this particular attribute is felt to be very harmless and unimportant. Position in time presents greater difficulties; as an attribute it invokes the same sort of suspicion as does a nativistic local sign.

The importance of these two attributes must be emphasized at this point. For the question of the simultaneity or succession of bodily and mental correlatives is bound up with them, and unless this alternative is resolved, there can be no hope, at least from the physical side, of

extending the theory of psychophysical correlation into a theory of psychophysical continuity. Correlative simultaneity is incompatible with the general principles of physical science and especially with the conception of energy, if we ignore for a moment the difficulty of considering consciousness as a form of energy at all. And if succession be the more probable view, the further question arises: is the succession of bodily and mental states of only one direction, or is it also reversible? But it must be abundantly evident that the alternative of psychophysical simultaneity or succession is not really resolvable. We can hardly hope to succeed in comparing the conscious position in time of two such events as an experience and its accompanying neural excitation. And even if we could, it would avail us nothing. For the presence of a regular interval of time between neural excitation and correlative experience would be absolutely undetectable. As a matter of fact an interval of time does elapse between stimulation of a receptor and the correlative sensory experience; but we are quite unconscious of it and fail to detect it, unless we infer it from the fact that the time interval for different receptors differs or from the latent time of muscular reaction.

Thus it is evident that we must include duration and position in time amongst the attributes of sensation, even if only to provide a basis for the general problem of correlation formulated above. The problem of the alternative of simultaneity or succession does not follow upon the problem of correlation, but must be merged in it. Hence it is possible to omit all reference to time in the statement of the latter problem.

6. *Some difficulties urged against parallelism.* Some form of parallelism may then be said to be by presumption the accepted doctrine. But the more restricted view of interactionism is not without its supporters. It is, curiously enough, at once the easiest doctrine to make plausible and the most difficult doctrine to prove. To make it plausible it is only necessary to pause before the difficulties of psychological analysis on the one hand and to underestimate the possibilities of neural complexity on the other. None of us can really avoid doing either of these things at some point or other. The difficulties of psychological analysis make us incline to believe that the brain sometimes yields us full-grown 'higher' mental states that are unanalysable, or weaves into a unity components that could not be supposed to produce that unity entirely by themselves. And if we succeed in our psychological analysis, we may perhaps too readily

concede that the brain could not possibly contain mechanisms of that peculiar kind which our analysis demands. Those who are sceptically or critically inclined will, therefore, find it hard to abandon the parallelistic view, however strong the evidence against it may be.

This evidence has been recently gathered by W. McDougall and has been admirably expounded in his works, especially in his paper "On the Relations of Corresponding Points of the Two Retinæ<sup>1</sup>," and in his book *Body and Mind* (Methuen, London, 1911). McDougall attempts with the help of the typical, and, of all, the best studied, example of binocular vision to show *that for certain aspects of psychical states—generally their unity amidst diversity of content—no physical correlative is known or conceivable*. I propose briefly to state his arguments and general conclusion in favour of interactionism, and to urge certain considerations which seem to me to make the facts still compatible with the demands for correlation and consequently with the broader views of parallelism.

*The arguments are as follows*<sup>2</sup>:

- (1) "Any illuminated surface appears no brighter (or but very slightly brighter) in binocular than in monocular vision." [Independence and equivalence.]
- (2) The facts of Fechner's paradox. [Reconciliation of differences.]
- (3) An after-image is much more easily revived by stimulation of the eye it was formed in than of the other. [Independence.]
- (4) Binocular flicker disappears at the same rate of alternation of phases as does unocular flicker and is practically independent of simultaneity or alternation of phases in the two eyes. [Independence and equivalence.]
- (5) The facts of flicker-rivalry and of the rivalry frequently observed in binocular colour-mixture and of the volitional predominance of either of two rival fields. [Failure or suppression of reconciliation.]
- (6) The independence of the two eyes with regard to the after-effects of seen movement. [Independence.]
- (7) The fusion of disparate points in binocular vision and the influence of practice thereon. [Reconciliation of differences.]
- (8) The acquired readjustment of corresponding points in certain cases of squint. [Ditto.]
- (9) "Perhaps the strongest evidence against the 'common centre' is afforded by the facts of functional blindness of one eye, whether occurring as a symptom of hysteria or induced by hypnotic suggestion." "But how is this dissociation or circumscription effected? The subject himself knows nothing of the anatomy of his brain<sup>3</sup>."
- (10) "In certain rare cases a lesion of the visual cortex has produced a small area of blindness in one retina only: a fact fatal to the common view<sup>4</sup>." [Independence.]

<sup>1</sup> *Brain*, xxxiii. 371 ff.

<sup>2</sup> *Op. cit.* 372. The words in square brackets are added by me.

<sup>3</sup> *Op. cit.* 3, 2.

<sup>4</sup> *Op. cit.* 292.



(11) "It seems that the owl, the frog, the chameleon, and other beasts of prey enjoy binocular vision in spite of the fact that in them the decussation of the optic nerves at the chiasma is complete."

(12) "The hypothesis of the 'common centre' is founded upon a radical misconception of the conditions of fusion of effects of sensory stimuli"—viz. "That the sensation evoked by the stimulation of any sensory point or nerve-fibre comes into existence as an isolated or detached fragment of psychical existence, and that such fragments become compounded to form a consciousness" only in virtue of a corresponding fusion of subservient nervous conditions. The true statement may be formulated in the following way: "In so far as sensory stimuli affect consciousness, they produce partial modifications of the complex but unitary whole of consciousness; and when several stimuli simultaneously affect consciousness, their effects in consciousness can only be discriminated from one another in so far as there obtains some special ground of distinction. Such special grounds are of two principal classes—namely differences of quality and differences of local signature of the several sensory effects of sensations, as we commonly call them; and the power of distinguishing...sensations-elements by aid of either of these grounds of distinction depends largely upon previous practice in active discrimination."

"When the effects of two or more sense-stimuli appear in consciousness combined to a common resultant, this is because the separate cerebral processes act upon this one being [call it the soul or what you will] and stimulate it to react according to the laws of its own nature with the production of changes in the stream of consciousness<sup>1</sup>."

I think we may agree that these arguments are decisive against any view that holds that the excitations from the two retinae impinge upon a unitary, common, centre—a sort of blob of undifferentiated jelly—and are there simply and entirely summated, merged or wrought into one another, as two drops of water that run together. But this view hardly needs such heavy condemnation. For if the unity of the individual consciousness does not involve a punctate cerebral seat for the soul—as the failure to find one seems to show,—neither should the lesser unities of consciousness necessarily imply the existence of corresponding punctate centres. Besides, it is obvious that the existence of such punctate centres of fusion would be the very strongest evidence against parallelism. Even if we suppose that in the common centre the two contributory excitations were merely superimposed without summation, such a centre would be useless, because it would offer no physical correlate to binocular stereoscopic vision. The neural basis of binocular vision is undoubtedly much more complex than has been often rashly supposed.

But having established that the two eyes are in certain respects functionally independent, in others functionally equivalent in spite

<sup>1</sup> *Op. cit.* 298.

of independence, in others again irreconcilable, and, finally, in others without doubt extensively and variably reconcilable, both with reference to the accompanying experiences and to the muscular outcome,—must we therefore conclude that the demands of no legitimate form of parallelism can be fulfilled? Are we not rather called upon first to systematize as concisely as possible the very various psychological facts adduced, and then to find or conceive some correlative neural basis for them. If we cannot pull the child's coat on to the man's back, we must cut a new garment from our cloth.

7. *The problem of psychical analysis.* It is a common objection which McDougall urges, that sensations of similar quality fuse in a way that defies analysis. They can be distinguished when they occur successively merely because of their succession. But we must be careful that we do not try to dictate the facts. One obvious limit is set to analysis. We cannot maintain binocular synthesis and at the same time somehow separate it for observation into its discrete parts. But that does not mean that when fusion of sensations takes place, there is no longer any evidence of the existence of the manifold that fused to unity. The fused unity does not differ radically and in every respect from its components, although suggestions are sometimes given to this effect. The rather narrow limits set to the reconciliation of differences in fusion show this most emphatically.

8. *The laws of psychical fusion.* We must simply recognise the peculiarities of psychical fusion. We must not expect them to be the same as those of physical fusion. The law of the conservation of physical energy involves the summation of fused components; it is in fact a quantitative law. If there is anything of which we may be certain it is that the laws of psychical fusion are not quantitative laws. Can we not recognise a law which might boldly be called the law of the conservation of psychical identity, and which might be formulated in the following words?—When two sensory experiences combine so as to produce a unity in which they are not separately distinguishable, all those attributes of which the same varieties are common to both experiences, are conserved identically, without prejudice to any divergent psychical attachments these identical attributes may possess. I know of no attribute of experience which offers any exception to this law. And I do not at all see that on any view whatsoever demand could be made for the existence of a parallel unification or identification of subservient neural processes. The psychical identification as such is quite compatible with physical

discreteness as such. Physical discreteness of the neural processes which actually subserve equal intensities psychically identified (if we have really established such a thing), would only involve the assumption that the neural correlative of intensity is not the same part of the neural unit, say a series of neurons, which subserves a given sensation, as perhaps the neural correlatives of colour-quality or local sign of that sensation. If the fused psychical complex is subserved by a coordinated neural complex, why should not the simple sensation of many concentrated attributes be subserved in the same way?

But the main interest of the systematic psychological study of binocular vision resides, not in this law of the conservation of psychical identity, but in the laws which govern the fusion of experiences in so far as they differ from one another in some one attribute. In binocular vision unocular fields which differ in quality, intensity, or local sign are reconcilable under certain circumstances. Differences of quality are sometimes reconcilable, when we get the processes of binocular colour-mixture. Differences of intensity often produce lustrous effects and differences of local sign usually give us stereoscopic vision. All of these binocular effects can also be got unocularly.

9. *The visual systemic sign.* Moreover, we must not omit to take note of the individual nature of the two fields of vision. It is a familiar fact that if we interchange the right and left eye views of a stereoscopic slide of a simple object such as a pyramid, the previously solid pyramid pointing its apex towards the eyes now seems to be a hollow pyramid whose open base is exposed to view. In both cases accommodation, convergence, and the sum total of excitation are the same. The only difference is the interchange of the two halves of the complex excitation. It is therefore clear that the two eyes or the two fields of vision are not indifferently interchangeable systems. We can, I think, hardly assume that the mere disposition of the cerebral components of excitation produces such a radical reversal of psychical effects without any intermediary being present in the unocular components of fusion. I propose to call this intermediary—for want of a better name—the ‘systemic sign.’

If this sign is conscious it should be introspectible, but it need not be readily so. Its existence is sufficiently guaranteed if it is detectable at all. I think this may be allowed, although the evidence is hardly yet clear. Apart from this, however, and as its existence seems to me to be implied as the ground of binocular reversals, I should like to



suggest its use as a means of explaining the facts of functional blindness referred to by McDougall in his ninth argument. These facts involve a physiological knowledge of areas of localisation only in the eyes of a theory which ignores or refuses the assumption of systemic signs. If there is a visual systemic sign, it is evident that a patient in hysteria or in hypnosis has a direct means of bringing the field of vision of one eye into relation to suggestive inhibition. The inhibition will naturally apply to all the contents of consciousness that are characterized by the systemic sign; this in turn may naturally be aroused by verbal suggestion or by touching one eye or the like. For it is evident that what is associated with a systemic sign can act as a vehicle to reach the contents of that system rather than those of another. To each visual system we must suppose to be linked in complex ways the muscular and tactual sensations from the same eye, as well as all the conceptual terms applied in intelligent life to these.

May we not further consider the systemic sign as the psychological correlative of the separate central localisations of the effects of stimulation of the two retina? I can see nothing but advantage in doing so.

When the contents of the two fields of vision are identical, the difference of systemic signs seems to be somewhat ineffective. The result of fusion gives chief prominence to the conservation of identity, except in so far as the appearance of flatness is concerned. But it would be rash to assert that when the fields of the two eyes are not in any way distinguishable, binocular vision is then completely identical with unocular vision. There has been a tendency, I think, to presume upon this view in treating of the relations between unocular and binocular intensities and the like. If there is no summation of intensities in binocular vision, it does not follow that the unitary binocular field which results is a unity without any included differences. It is only so as regards intensity. The same holds for binocular colour-mixture and for other attributes of visual sensations.

On the other hand, when the two fields of vision are extremely disparate in any respect, the systemic signs become again ineffective; they fail to reconcile these differences and rivalry results, be it rivalry of intensities, colours, or local signs. Reconcilable differences must lie apparently within a certain range, which may vary for each kind of difference, as well as for practice; and certain forms of reconciliation exert a strengthening effect upon the reconciliation of otherwise rival fields, as we find in lustre and binocular colour-mixture, which are both supported by identity of contours and by stereoscopic vision.

10. *Sufficiency of positive statements.* But it would be inopportune to pursue further matters which still await exact determination and formulation. Perhaps enough has been said to indicate methods which seem applicable to the systematization of experience. In face of the statement that a reasonable and sufficient analysis of experience is impossible, more than an indication of methods can hardly be expected. In so far as systematization succeeds, we do not need to go beyond the positive statement of the laws included within it. The law of the conservation of psychical identity does not seem to call for any operative agent. I do not see any reason why we should expect exact statements of the laws of the reconciliation of differences in fusion to involve the assumption of any intermediary. The 'soul' seems an unnecessary postulate, at least within the sphere of sensory experience. Whether it is necessary in other regions of experience it is at present impossible to say with positive assurance. But in so far as these regions of experience present features analogous to those of sensory experience, the postulation of the soul as the agent of the unification of experience hardly seems inevitable. For this purpose the soul seems to be as unnecessary in mental life as are in nature the agent 'forces' we so often tend to assume as the motive life of her laws.

But although the soul does not seem to me to be in this connexion as necessary and as useful as McDougall would have it, I am sure that in postulating the soul to explain the processes of fusion McDougall has emphasized the problem which on the psychical side first faces every attempt to solve the general psychophysical problem. There can be no doubt that the problem of the understanding of the psychical complex as such is in this connexion chief of all. It would be rash in a short paper like this to presume upon the solution of it. I can only indicate the lines of solution which at present seem promising and worthy. I do not think that we can yet afford to accept any conclusion that the satisfactory analysis of psychical complexes is impossible. Nor do we seem to be helped by postulating a most complex and wonderful agent to relieve us of our difficulties. We must face these difficulties boldly and hope for success in the positive systematization of experience and in the understanding of its complexes in terms of its elements. A quantitative understanding of them is excluded, it is true; but have we therefore in the psychical world no form of insight which convinces and satisfies? Surely we have! If we can spread out the physical world under the microscopic eye of science and gaze upon it through that vision with the full promise, if not already the gift of

satisfying conviction, can we not also hope to spread out the psychical world before us and leaven it all with the yet partial insight of our inner vision? We still see only with the eyes of childhood; we have not yet observed and pondered enough. But the practical reason of childhood is already the promise of man's purest understanding.

11. *Psychophysical correlation.* When we turn to the problem of correlation, we must ask at each stage in the progress of psychical analysis whether our results are compatible with what we know about the central nervous system from evidence other than psychological. It is of course permissible, where direct knowledge fails, to speculate on the probabilities of neural arrangements by inference from psychical facts. We might even under certain circumstances speculate upon the probabilities of a wider world of mind from consideration of the larger schemes of the physical world. But it should surely make us call a halt when we find that inferences made from psychical facts to physical probabilities are held to establish a partial lapse of the expected parallelism between the psychical and the physical. Even if we admit the validity of these inferences, must we not agree that they do not in principle really carry us beyond the knowledge of every-day life, that there are two eyes, but only one mind and one muscular response? We cannot suppose that the neural paths from the two eyes are entirely separate over their whole course, but only that they are not coordinated so soon or at the same points as we once believed they were. For coordinated they must be somewhere and somehow, not for the sake of psychical fusion, but merely for the sake of the unity of the muscular response.

*This coordination of the muscular response* is recognised by many as a fact of unique importance. But its importance must not be exaggerated. It cannot be supposed that confluence in the efferent system is the sole ground of psychical fusion and explains all the peculiarities thereof. For we should then be involved in all the confusions of a 'common centre.' Confluence for the purpose of motor coordination can only be supposed to be a partial condition of fusion, if we consider that the neural substrate of, say, intensity lies where the converging paths are still separate, while the neural substrate of local sign lies near the point of confluence. The neural correlates of the various attributes of one simple sensation would then be spread out in some sort of linear series. No objection would, of course, be offered to such a view from the side of psychological theory. Our only enquiry is whether the demands of correlation can be satisfied or not. It is for the physiologist to say



upon the basis of direct evidence whether any proposed neural scheme, devised to explain the facts of muscular coordination or to meet the demands of parallelism, is possible, probable, or necessary.

It is a significant fact that the feature of uniocular experience which shows by far the greatest extent of reconciliation of differences in fusion is its local aspect. There is also clear evidence in the cases of lustre and binocular colour-mixture that the reconciliation of differences of brightness and colour is very much supported both by the identification of local signs and by the reconciliation of their differences. The coordination of muscular response seems therefore to involve the existence of a neural basis which shall bind the efferent system to, or give it some sort of foundation in, the neural correlative of local signs; so that the modification or complication of muscular responses shall help to modify or complicate local signs. In this way we might give credence to all the facts and still deny that the motor coordination is the sole ground of fusion. *It would be a condition of fusion only in so far as it is based upon, and by its changes helps to modify* (other means of modification still being possible) *the local signs*, which, as originally given, or as integratively developed, enter as the attribute of sensations into the components of a complex process of fusion.

Therefore it still seems possible to correlate completely the complex psychical unity of binocular vision, fused according to the particular laws of psychical fusion, with the complex physical unity of binocular stimulation and response, coordinated according to the particular laws of neural coordination.

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